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EDITORIAL.

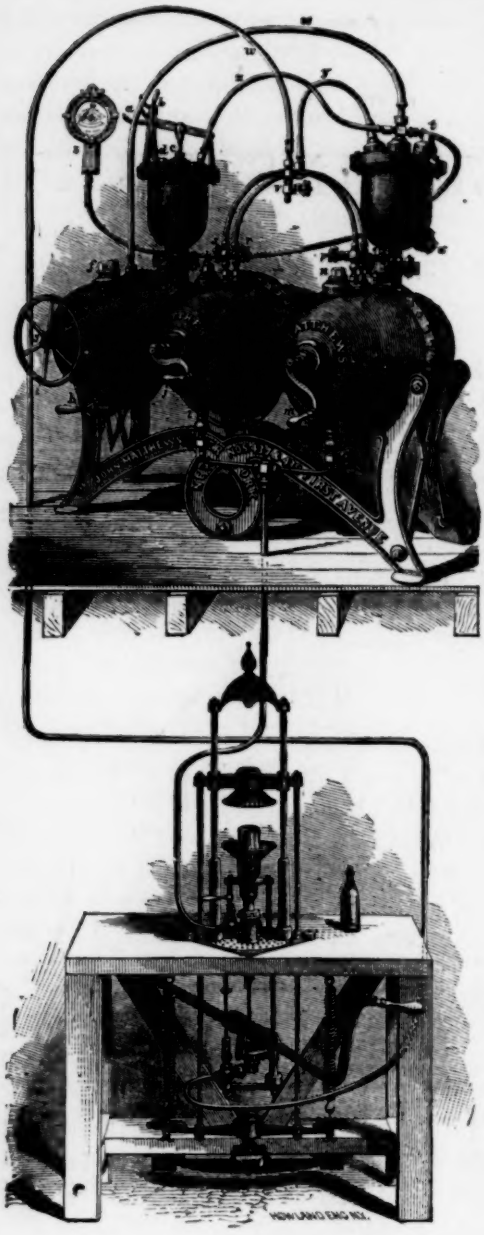
ITEMS AND NOVELTIES.

Soda Water Apparatus—continued.—In our issue of October, we described several of the excellent machines in connection with the above subject, which had been devised and were now employed by Mr. John Matthews, of New York, and we then promised further particulars for our next number. A portion of the present notice, was, in fact, in type, but was crowded out by other matter which came in late, and yet with an imperative demand for admission.

Among the most important of the new arrangements devised and introduced by Mr. Matthews, is that for bottling aerated liquids under pressure; this apparatus, with the aid of the accompanying illustration, we will now explain.

In the usual method of bottling, the pressure of gas on the aerated liquid in the fountain, drives the water through a tube into the bottle. As the liquid enters the bottle, at first, under no pressure,

a large part of its previously-absorbed gas escapes, together with the air in the bottle from the weighted valve on the bottling machine. The escaping gas causes foaming of the liquid, which, becoming entangled with this escaping gas, causes large leaks, especially if wines or liquids containing sugar or extractive matter are bottled. Owing to the disturbance caused by the agitation and escape of gas, the liquid, when closed in the bottle, will frequently reabsorb a portion of the gas in the space above the liquid, causing considerable reduction of pressure. To compensate for this reduction, as well as for the excess of pressure necessary to open the escape-valve, so that the bottle may fill rapidly, the employment of a much higher pressure is required in the fountain than in the bottle.



The system here shown is designed to obviate these objections; and reports from the bottling establish-

ments having it in practical use, are conclusive as to its success. In this system there is no escape-valve at the bottling machine, and the aerated liquid is kept under constant pressure during the entire operation of bottling. All the bottles filled by this system are of equal pressure, and the same pressure as the liquid in the fountain. The fountain, or reservoir is elevated above the bottling machine, and tubes communicating with the aerated liquid, and also with the compressed gas in the fountain, descend to the bottling machine. The bottle, being brought to the filling machine, is first charged with gas from the upper or gas tube, and then communication is made with the lower part of the fountain, the liquid descends to the bottle, and there displaces the gas, which is thus returned to the fountain.

The opening of the gas-valve is automatic: by simply raising the hand-lever, which elevates the gas-tube in the bottle, the communication is opened with the compressed gas in the fountain. If the bottle is filled in an inverted position (as when gravitating stoppers are used), the gas-tube is raised in the bottle to the height to which it is desired to fill the bottle. This ensures filling the bottles all to the same height, and prevents over-filling and consequent breakage. Still liquids, or liquids not yet having generated carbonic acid gas by fermentation, can be readily filled into bottles by this system without exposing them to the action of atmospheric air.

If it is desired to bottle them in bottles requiring an external pressure to sustain, or to close the stopper, the barrel or other reservoir containing the liquid may be separated from the gas reservoir, and, after drawing off the liquid into the bottle, a small quantity of compressed gas may be admitted by depressing the hand-lever and opening the appropriate valve. This system, for liquids under pressure, and for still liquids, is now in successful practical use in a large number of establishments in the United States, and in several in foreign countries. The saving of materials used to generate the gas is estimated to be fully 30 per cent., as compared with the usual method, and as the beverage does not lose its absorbed gas by expansion to a lower pressure than that in the fountain, the low pressure employed in this system produces equally good results, besides avoiding much of the breakage and other losses inseparable from the usual method.

Another of the ingenious contrivances in use by Matthews, is the

arrangement shown in the accompanying cut, which may be described as follows:

A large ovoid glass vessel, having a capacity of 5 to 10 gallons, is provided with a strong iron casing, capable of safely supporting any pressure to which the apparatus would ever be subjected. In this casing the glass vessel is supported by large blocks of sheet rubber or other elastic packing A delivery tube is also attached, as shown in the cut, and descends nearly to the bottom, while communication is made by means of a small orifice at the top, between the interior of the glass vessel and the space between it and the exterior shell. By this means the pressure is equilibriated on the glass vessel, and is wholly sustained by the exterior case.

The advantages of this arrangement for beverages under pressure, which are to be kept on hand for a long time, are too obvious to need comment. We will only remark, that a large number of these fountains are in successful operation.

Quincy Railroad Bridge across the Mississippi in Illinois. We see, in the *Chicago Tribune*, an account of the opening of this bridge, which was tested and examined by the Engineers, Directors and others, on the 7th of November. We subjoin the following

account of this work, for which we are indebted to the above journal.

The Bridge Company was organized on the 20th of November, 1866, by the amalgamation of two incorporated companies, one chartered by the General Assembly of Illinois, and the other by the Legislature of Missouri. It took the name of the "Quincy Railroad Bridge Company." Nathaniel Bushnell, of Quincy, was made President; James F. Joy, of Detroit, Managing Director; C. A. Savage, of Quincy, Secretary; A. T. Hall, of Chicago, Treasurer; N. Flagg, of Quincy, General Agent; J. L. Lathrop, of Hannibal, Auditor; W. Colbern, of Toledo, Consulting Engineer; and T. C. Clarke, of Chicago, Chief Engineer.

Surveys were made, the bridge located at once, and the work commenced. The main bridge, that which spans the main branch of the river, consists of two draw spans, 160 feet each, making the length of the draws, or rather, swing, 360 feet. Two spans of 250 feet, three of 200, and eleven of 157 each, making a total, with the mason-work, of 3,250, constitute the main bridge. The embankments and trestle-work between are, 1,400 feet in length; bay bridge, 613 feet; one draw, 190 feet long; and four spans of 85 feet each. Total length of the bridge and embankments, from the Chicago, Burlington and Quincy to the St. Joseph tracks, nearly two miles. The bridge is elevated ten feet above high-water mark, and twenty feet above low-water mark, on stone piers, the stone coming from Grafton, Mo., and Hamilton, Ill. The superstructure is entirely of iron, and on the Pratt truss principle. The masonry and foundations are the work of the Bridge Company, under the immediate direction of T. C. Clarke, Esq., Chief Engineer. The total cost of the bridge is $1,500,000. The bridge is so proportioned that a train of two locomotives and the heaviest freight cars strain the iron only about 7,500 pounds to the inch, while its ultimate strength is 60,000 pounds to the square inch. Strength, before any permanent stretch begins to be seen, 28,000 pounds to the square inch. Every piece of wrought iron in the ties, links, bolts, &c., was tested in a hydraulic press up to 23,600 pounds to the square inch, and struck with a hammer, while under tension, before being used in the bridge. The first stone of the structure was laid September 25th, 1867, and the last stone August 5th, 1868. It was opened for traffic November 7th, 1868. The season of 1867 was such as to prevent the commencement of the foundation before

September, 1867. It should have been begun in June of that year.

On the occasion of the above-mentioned testing and examination of the bridge, a report was drawn up and signed by the various engineers present, and this we here quote in full.

"QUINCY, *November 7th*, 1868.

"The railroad bridge over the Mississippi, at this place, is now completed, and will be put in use for freight and passengers on Monday next. It has been, to-day, subjected, in the presence of a party of engineers and others, to various tests, the results of which are as follows: Three of the heaviest locomotives were coupled and placed at rest centrally upon the span 250 feet long, and the deflection or yielding of the bridge very accurately observed by means of instruments. The total weight of the load was 300,000 pounds, and the maximum deflection at the centre of the span was 2·4223 inches, being one-sixteenth of an inch less than the deflection previously calculated.

"The same load was then placed upon a span 157 feet long, and a deflection produced of 1·375 inches, which varied but little from the result of previous calculations.

"The three locomotives, still coupled, were then run over the 157 feet span several times, at rates of speed varying from ten to sixteen miles per hour. The deflection produced was 1·406 inches, being an increase of only 0·3 inch over the deflection while at rest. Probably no severer strain than the above will ever be applied to the bridge in actual use. In each case, on the removal of the load, the bridge at once resumed its previous form.

"A few words of explanation of the above experiments may be interesting to the public. Short of the dangerous and expensive process of actually breaking down a bridge by the weight equal to its ultimate strength, the only method of proving its safety is to measure the deflections produced by stationary or running loads. If these do not exceed the deflections, calculated as due to the known elasticity of the material, it may be safely inferred that the bridge is free from dangerous defects, either in material or workmanship. The strain applied to-day was 10,000 pounds to the square inch of wrought iron, and 5,800 pounds per square inch of cast iron.

"On the 157 feet span, the strain applied was 9,000 pounds per

square inch on the wrought iron, and 10,200 pounds to the square inch on cast, being about one-quarter more than the strain produced by the passage of the heaviest freight trains. All the wrought iron had been tested, before being used, by a strain of 23,000 pounds per square inch. Specimens of the wrought iron which were subjected to the ultimate strain, broke only at from 60,000 to 80,000 pounds per square inch. C. Shaled Smith, St. Charles Bridge; O. Chanute, Kansas City Bridge; J. E. Ainsworth, Dubuque Bridge; R. H. Temple, St. Charles Bridge; J. B. Moulton, North Missouri Railroad; D. C. Janne, Keokuk Canal; G. H. Morison, Kansas City Bridge; G. H. Nettle, Chief Engineer H. and St. Jo. R. R."

A Round-About Railroad(?)—In looking over an article on the Locomotives of the New Jersey Railway, published in the *Engineer*, (London) for October 16th, we were quite surprised to discover a peculiarity in the alignment of that road that we were not before aware of. The article states: "That whatever English engineers may think of the general design of these engines, it cannot be denied that they work well over bad roads. They work, for example, at high speeds, round *one stretch*, almost a *continuous curve* $11\frac{3}{4}$ miles long, the radius varying from 550 feet to 1,500 feet, with a steady rise." Now, taking the radius of 1,500 feet, the engine would move in a complete circle in every $1\frac{785}{1000}$ miles, and the line would double on itself a little over six times in a distance of $11\frac{3}{4}$ miles. We have travelled this line many times, and must confess to great want of powers of observation, that we should be obliged to depend upon an English journal for this important information. We shall take particular pains to notice it on our next trip. New Jersey may well be proud of her modern Tower of Babel. May her sands never overwhelm it, as did the sands of Asia her ancient representative. J. M. W.

Friction Clutch Pulley.—By Brown & Sharpe Manufacturing Company. A friction pulley that will operate with certainty, and without noise, that will continue to operate well, even after it has experienced the necessary wear of long-continued use, and which is not liable to derangement, by which work is delayed, and time lost, is a most desirable acquisition.

From the testimony of many machinists in this city, and also from an examination of the principle and construction of the apparatus named above, we believe that it fulfils the desired conditions in a very desirable manner.

The merits of this instrument will, we think, be evident from the following description of its structure.

Fig. 1.

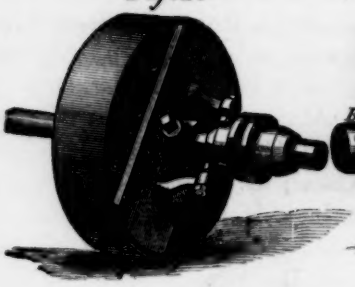


Fig. 2.

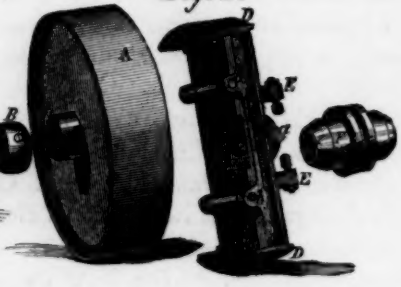
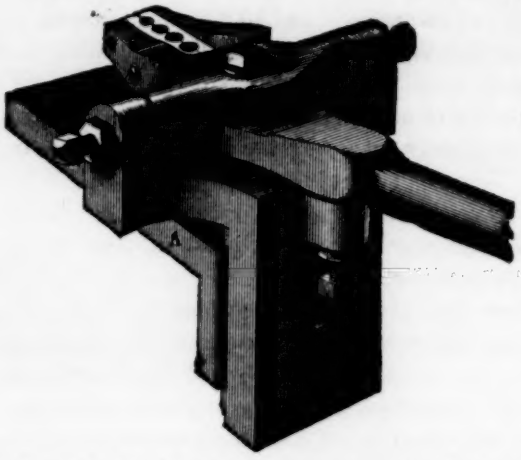


Fig. 1 represents a complete Friction Clutch Pulley in working position upon a shaft. Fig. 2 represents the parts of the same; A is a pulley, the inside surface of the rim of which is turned. This pulley revolves freely upon the shaft, and is kept in position on one side by the collar, B, and on the other by the segment plate, C. The segment plate, C, is fastened to the shaft by the set screw, *a*. Attached to this plate, and sliding in planed grooves, are two segments, D D, which move in opposite directions, at right angles, to the shaft. The outer surfaces of these segments are turned to the same diameter as the inside of the rim of the pulley, A. The two levers, E E, are connected to the segment plate, C, by pins passing through them and the ears, *b b*, which act as fulcrums. These levers pass through and are fitted to the segments, D D, and also through the segment plate, C. In the outer ends of these levers are adjusting screws with set nuts. Fitted to and sliding upon the shaft is a thimble, F, the end of which is turned of a conical shape. Upon the outside of this thimble is a groove into which a shipping fork is fitted. It will be readily seen that when the thimble is pressed forward, toward the pulley, the conical end comes in contact with the rounded heads of the adjusting screws, by which the two levers, E E, are forced outward, carrying the two segments, D D, which movement brings the faces of these segments into contact with the inside of the rim of the pulley, binding the surfaces together, and thus communicating the motion of the pulley to the shaft. This pulley is perfectly noiseless as well as simple and efficient, with no liability of locking or unlocking, except at the will of the operator.

Screw Slotting Machine.—By Brown & Sharpe Manufacturing Company. Quite an extensive machine is often used by gunmakers and others for slotting the heads of screws, but the device shown below, which can be attached to an ordinary hand-lathe, is believed to be more efficient for the purpose than any machine heretofore made. A single bolt fastens the platform, A, of this apparatus to



the bed of a hand-lathe, the long lever projecting in front at a right angle with the bed. An arbor carrying a circular cutter is held in the centres of the lathe. The long lever is moved horizontally to open the jaws for inserting and removing the screws, and downward to bring the screw to be slotted against the saw. A stop-screw, B, governs its downward motion, and thus regulates the depth of slot in the screw-head. The working part of the apparatus can be raised or lowered on the platform front by means of the bolt, C.

Erricson's Solar Engine.—As various eroneous expositions of the results reached by Mr. Erricson in the new direction to which he has of late turned his attention have been published, we take occasion to give the substance of what he has himself enunciated in the Stockholm *Aftenbladet.*

He says that by concentrating the sun's rays with apparatus which he has not space to describe, and applying the heat so developed to steam and air engines of special construction, an amount of force can be obtained, which may be thus expressed. The accumulated solar radiation which falls upon a space 10 feet square or 100 square feet in area, can develop more than one horse-power.

This area is, of course, perpendicular to the direction of the rays.

The sun-heat wasted on the roofs of Philadelphia would operate 5,000 steam engines of 20 horse-power each, while the sun shone.

The area of one Sweedish square mile (=49 English), if only half covered with sun machines, would develop force enough for 64,800 steam engines of 100 horse-power each.

At the surface of the sun an area of 10 square feet must emit heat enough to run an engine of 45,984 horse-power.

A Tangential Water Wheel, or Turbine, with partial admission, has lately been constructed by Messrs. Gwynne & Co., London, to take the place of one which had become useless through age, but which was originally set up by the Moors, at Molino del Rey, near Granada, in Spain. The culvert which conveyed the water to the wheel, the wheel-pit and the tail-race, were all cut in the solid granite, the entire work indicating great skill and judgment on the part of the original constructors.

In the wheel now introduced, the water is admitted to about one-quarter of the circumference by nine guide-buckets, which may be closed, one after another, by a valve which forms part of a circle and moves in guides cast on the casing. By this means provision is made for utilizing the very variable water supply which exists at this place, without loss of useful effect. From $1\frac{1}{2}$ to 12 horse-power can be obtained, according to the quantity of water that may be found.

Water inclosed in Glass.—At the last meeting of the Institute there was presented by Mr. James Gaffield, of Boston, a piece of glass, being the head of a stopper containing water in its interior cavity. This was one of a large number of similar specimens found after a fire in which a great quantity of like objects had been destroyed, and resulted from the cracking of the heated glass by the water thrown on the ruins, the entrance of the water as the object cooled, and subsequent closing of the fissure, which in this and other cases was hardly perceptible, and entirely precluded the egress of the water.

Editorial Correspondence.

CORNISH PUMPING-ENGINES.

MR. EDITOR:—In looking over the October number of *The Journal of the Franklin Institute*, I was somewhat surprised to find, from the communication of Mr. W. H. G. West, that my eulogy of the Cornish engine, for pumping purposes, had only proved, that the rotative engine was equally good, or perhaps better. This certainly was not what I intended. But as Mr. W. has been kind enough to point out the more prominent errors contained in the article referred to, an opportunity will be thus afforded me for making any necessary corrections.

The grand question, as I understand it, is whether the operation of pumping water be better accomplished by the direct, unfettered action of a certain pressure, or by the secondary action of the same pressure, shackled by the incongruous addition of a revolving shaft? In case this question is of too abstruse a nature, it may be put in a plainer form, by substituting the analogous one, as to whether the shortest route between two points diametrically opposite each other, on the circumference of a circle, be by proceeding round the periphery, or by going straight across? Perhaps I may be biassed in my opinion, believing as I do in the direct path, and will therefore not enforce again my peculiar views at this time, but will leave it to the readers of this *Journal* to decide this seemingly difficult question. Turning my attention to the minor points in the case, as prominently brought forward by Mr. W., I find, that having at one place, recommended a uniform speed of piston for pumping water, I have afterwards acknowledged that the speed of the piston, at the time it is *not* pumping water, is uniformly accelerated, *i. e.* making the in-door stroke. The next is a direct question. "If, in the Cornish engine, the momentum acquired, together with the force given out by the expanding steam, carries the piston to the end of its stroke, does not the *vis viva*, or potential energy stored up in the moving parts of the rotative engine, together with the expanding steam, carry the piston of that engine to the end of its stroke, and does not the *steam* perform the *whole* work in either case?" Certainly the *steam* performs the *whole* work in either case. But it is by no means evident that the *steam* or the *whole* work is the same in both cases, as I shall hereafter, perhaps, be able to show. Again: "The

Cornish engine passes through the steam stroke very rapidly. If the pressure at the end of the stroke equalled the resistance, the *vis viva* of the moving parts would shoot the piston through the cylinder head, unless prevented by the catches." That depends upon the manner in which the engine is operated. There is no doubt, if it were driven in the same manner as found necessary for rotative engines, such a catastrophe would, if possible, occur at every stroke. And herein lies one of the chief points of merit, characteristic of the Cornish engine, for it is a known fact, that when the piston reaches the end of its stroke, the force of the steam is not sufficient to balance the weight of the heavy pump plunger, which has been carried onward solely by the *vis viva* stored up in the weighty moving parts. It is by the paying back of this amount of power, borrowed at a time of superabundance, that the pressure is made to coincide with the resistance,—balancing the account at every stroke. It is the duty of the engineer to see that this is properly done without premonition from the catches.

"Mr. H. gives us this same thing as a fault in the numerous direct acting steam pumps, and in these words: 'the terminal pressure must invariably be fully equal to the load, or the pump will come to a dead stand.' Now why will not momentum help this unfortunate engine as well as the Cornish engine? It will. Mr. H. knows it, and is convinced, too, that the terminal pressure need be no more equal to the full load in one machine than another."

In this, I am afraid Mr. W. has paid me an undeserved compliment, as I cannot recall to mind having been possessed, at any time, with the knowledge of the facts attributed to me. One reason I remember to have had, why momentum would not help the unfortunate class of engines alluded to, was, that because being horizontal, they were not quite so vertical as the Cornish engine, and that the laws of gravity did not act upon each precisely alike. I probably held an opinion, in regard to the unfortunate machines, that the principal effect of the laws of gravitation, in their case, was to produce friction, which occurred to me as being somewhat antagonistic to inertia. From an extended experience in the premises, I have invariably found that when the pressure on the steam piston is permitted to become less than the load on the water-piston, at that particular instant, such pumps come to a dead stand, no effect from *vis viva* being at all discernible.

"On page 31, we find that the plunger descends with a uniformly augmented velocity, and the momentum acquired is again stored up

at the end of the stroke, in the steam contained above the piston, to be made available in the return stroke. The piston, and *therefore* the plunger, *cannot* descend in this manner, for it is brought up by cushioned steam, just as that of the rotative engine is, and the velocity of each piston varies from full speed to a stand still at each end. Where is the difference, and what ill effect would it have upon the engine, if Mr. Henderson's statement *were* correct?"

"The fly-wheel would give back all the momentum, or *vis viva* imparted to it, but the steam would not, in any engine."

In regard to the piston, Mr. W. is perfectly correct; it *cannot* descend in the manner described, as at that time it is going up. The equilibrium valve, however, *descends* before the plunger has completed its stroke, shutting in a quantity of steam, above the piston, which being compressed by the continued descent of the plunger, the augmented force is transmitted from the one to the other, to be made available in the return-stroke, as previously stated. The *difference* between this mode of action and that allowable in the rotative engine, *when applied to pumping water*, is very marked indeed. There are several reasons why that engine would not work so advantageously with the same amount of cushion. In the first place, it would not work at all, for it would be found in practice, that not until this cushion had been mainly got rid of, would the engine ever be persuaded to pass its centres; as every person who has ever tried that experiment, or the kindred one of giving lead to the valve, has invariably discovered.

In regard to the fly-wheel giving back all the momentum or *vis viva* imparted to it, Mr. W. is again correct; it unquestionably will give back all that has been imparted to it. But unfortunately for that valuable appendage, its share of the *vis viva* is but a limited per centage; for the bulk of it will be saddled upon the crank-pin, to be distributed through the journals to the pillow-blocks, thence to the bed-plate, until it is finally lost in the very foundation of the engine. A very simple method of arriving at the truth of this axiom, would be to take off the pillow-block caps of any engine, where they are bolted on in a line with the direction of the stroke. Of course, if the *vis viva* be imparted to the fly-wheel, the centrifugal and centripetal forces balancing each other, this can be done without any risk?

"Will Mr. H. call to mind that we find in general practice, much higher steam used in rotative engines than in the Cornish, and will he favor us with an estimate of the amount of power, in steam, lost

by this variable speed in any engine? None, I think. "Upon investigation, I find that from sixty to seventy pounds of steam is not an unusual pressure used in the Cornish engine, where a high degree of expansion is employed. Quite a high pressure for large condensing engines; it is questionable whether it would be politic to imitate it in the rotative engine, which cannot yield to any of the extreme variations of steam pressure, to which the piston is constantly subjected, owing to the obstinacy of the fly-wheel; the amount of power lost, in consequence, is, strictly speaking, that amount of *vis viva* which is expended upon the crank-pin at each alternate stroke. It will vary with the pressure of steam employed, the point of cut-off, and the load. The only reliable, or practicable way to estimate it, would be to erect two engines, of the same calculated cylinder capacity; one a Cornish, and the other a rotative pumping engine; the extra amount of work performed, with an equal quantity of fuel, by the former engine, would represent exactly the amount of power lost. That it is something considerable, may be ascertained in a more simple manner, by constructing a rough model of the working parts of a steam engine, and operating it by turning a crank by hand; then attach a sliding weight of the same gravity as the moving parts, to a return-crank proceeding from the end of the crank-pin, giving the same throw as the crank, in the adverse direction, so that when the piston is moving in one direction, this counter-weight will be moved diametrically in the opposite; then by turning the crank again, it will be at once very apparent that considerably less power is required than before. In the former case, the amount of *vis viva* parted with at each dead point, if the machine be run at a high rate of speed, is so great that it would be difficult to hold it, to test the experiment; while in the latter case, it will stand as steadily as though it were not under operation. This information I have from actual experiment.

"No *serious evil* effects can result from increasing the speed of the plunger near the middle and *towards the end of the stroke*, for when the water is put in motion, the plunger may move faster. Were the steam shut off, the *vis viva* of the moving water would carry the engine some distance with it, and the speed may, therefore, increase without loss, keeping the pressure against the plunger uniform. There is no loss here from variation of speed, except by friction; frictional resistance varies as the square of the speed, at low speeds, but as a *higher power at higher velocities;* and as the Cornish engine comes in like a rocket when it takes steam, we may

infer that it is the least economical of engines, as far as that *serious quality* is concerned."

Mr. W. is unfortunate in his selections for criticism, and certainly does not manifest any remarkable merit as a *careful reader*, personally, for it will be observed, the *serious quality* here complained of, has no existence in fact; for at the time selected, the pump is only taking water, and *no pumping* is being done. The rocket principle, on the contrary, is a manifest advantage, for the quicker the plunger is lifted, the oftener will it descend in a given time. This principle of variable velocity is also used with eminent advantage on the planing machines used in machine shops. The pressure against the plunger is the constant area of it; multiplied by the head, and by the *velocity*, it would not be safe to calculate upon any assistance from the *vis viva* of the moving water, under the circumstances stated. Pumping water is very unlike driving rotating machinery, which will continue to run some time after the steam is shut off from the engine; under the same operation, a pumping engine stops momentarily.

"We now pass to the saving of steam from loss by clearance and steam passages, and the isolation of the working end of the steam cylinder from the cooling influence of the condenser. Isolation in this case, means a high degree of expansion, or very high temperature to very low temperature, during the in-door stroke; then this low temperature, very little above that of the condenser *to* both sides of the piston during the out-door stroke; then exposure of all the cylinder but about a foot of the out-door end, to this same temperature; and last, the exposure on the lower side, during the in-door stroke, to the cooling influence of the condenser. This is saving by isolation with a vengeance. It is very well known that the pistons of Cornish engines, perfectly tight at one end, leak badly at the other, when the cylinder has no casing, or when the casing is cold."

The meaning of the term "isolation," in its application to the Cornish engine, as rendered by Mr. W., fortunately cannot overcome the incontrovertible fact, that however much of that engine is exposed to the frigorific influence of the condenser, there is twice as much exposed in the rotative engine; the one being *single*, and the other *double* acting. It is not necessary to multiply words over such a palpable theorem, nor is it of any consequence to ascertain by what mode of calculation it can be made to appear that less than the half is greater than the whole, or that 2 and 2 being four, 1 and 1

are 5 or 6, or as much more as prejudice may assume to make it. It never occurred to me what the probable effect would be, if the cylinder had no jacket, or having one, the engine might be worked with it cold, never having heard of any such contingencies in practice.

The remainder of the questions, pertinent to the subject under consideration, are more or less intimately connected with the foregoing, and have been fully met in my previous remarks, and in some that have been introduced by Mr. W. himself, on page 264. Amongst other advantages accruing to the Cornish engine, he there states "Expansion is carried to the fullest economical extent." Such being the case, as unquestionably it is, I should like to be informed how, in general practice, much higher steam is used in rotative engines?" as maintained on page 259, same article. The advantages of the steam jacket are also produced in *glowing* terms. "Steam condensed in expansion, or in the pipe by radiation, and water carried over by steam from the boiler, if it ever is, is re-vaporized by heat from the jacket, when the temperature of the steam in the cylinder falls below that in the jacket." There is an evident discrepancy between this statement and the following one, which appeared on page 262. But as the former is undoubtedly correct, the latter it must be presumed, is a typographical error.

"Steam jackets may be applied to any engines, even to locomotives, and would be, too, if practical men thought it would pay. The coal would do much better duty were the hot gases from it turned into the jacket, instead of being employed in evaporating water, half of which is condensed again before the remainder reaches the jacket."

In conclusion, I would add, that in order that this controversy may result in some permanent advantage to the inquiring public, aside from the mere opinion of Mr. West and myself, that as he may be possessed of statistical evidence corroborating his assertion that "many rotative engines do as good duty as the best Cornish engines" (page 262), to produce one to compare in efficiency with the celebrated Fowey Consuls experiments, where a duty of 130 millions of pounds of water was raised (by a Cornish engine) one foot high, by the consumption of 112 pounds of coal, *vide London Engineer*, July 17, 1866.

W. M. HENDERSON,
Hydraulic Engineer.

Philadelphia, November 14, 1868.

Civil and Mechanical Engineering.

BLASTING WITH NITRO-GLYCERINE AT THE HOOSAC TUNNEL.

We have been in hopes of giving our readers before this a full account of operations in the above direction, from the pen of Mr. Geo. M. Mowbray, who has been engaged both at the Hoosac Tunnel and elsewhere in the manufacture and use of this efficient explosive. Excessive press of business has, however, as yet prevented Mr. Mowbray from fulfilling his promise to us, though he assures us of a speedy supply, and, in the meantime, sends us the following extract from the *North Adams Transcript*, which, though not very precise and formal in its style, will, nevertheless, possess great interest for our readers, and will make up, by the vividness of its description, for any fault of form.

Monday, March 2, A. D. 1868, ushered in a snowing, gusty day; the wind, during the preceding night, had been urging puffs of snow, dry and crystalline, through every cranny of the mountain shanty, before whose soapstone stove I had been warming my rheumatic limbs; and, since travel seemed impracticable, I made a virtue of necessity, and accepted an invitation from my host to descend the west shaft of the Hoosac Tunnel, where the temperature, 60° F., would at least be more agreeable than on the mountain side, where the thermometer was then 6° below zero.

So, donning a miner's suit, rubber boots, Cape Ann oilskin jacket and southeaster, we stalked through the deep drifts of snow, and at 7, A. M., I found myself standing on the cage that is used for lowering and hoisting in the shaft, beside two pails, each having an inner lining of plate tin, with cover, suitable enough, as it seemed to me, to carry down hot coffee for the miners. These pails, and a conductor's lantern, were in charge of a man equipped in miner's costume, similar to our own, who was exchanging remarks with the topman, whose duty it is to signal the movements of the hoisting apparatus.

A gong sounding, we began to descend rapidly, or rather, as it seemed to me, the shaft began to rise around us in a most alarming manner.

The cold air of the outer world, descending and mixing with the

warm, saturated air rising from the tunnel, caused a vapor that rendered the light of the miner's lantern scarcely visible at two paces distance. It is an unpleasant position for a stranger to be in, going down, down, down, with streams of condensed vapor pattering on the head, neck and shoulders; and to relieve the monotony and suspense of the descent, I addressed myself to that man with the "hot coffee" pails.

"By the way, I thought I caught the word 'glycerine' spoken by that man who let us down."

"Possibly."

"Have they ever used nitro-glycerine in this tunnel? I mean that terrible explosive agent, which tears everything to atoms. I should like to see some of it, and know all about it; it would give one a sensation that would relieve a fellow of this—this oppressive feeling."

My companion deliberately lifted the cover of his pail, and taking thence an open slender tube, which seemed to contain clear water, said:

"There it is."

"What! Good —, in this cage? Do you mean to say we are boxed up in this hole with—?"

"Yes," returning his tin cylinder to the pail, and replacing the tin cover, "that is nitro-glycerine—one of twenty cartridges we are about to use in blasting."

I reflected; here I was, in a box four feet by three feet, no escape from a pail containing enough nitro-glycerine to send us up that shaft, and into eternity for the matter of that, and I had been confounding the "perilous stuff" with hot coffee. There was no help for it now, and as the heavy beat of the steam pumps and warm temperature rendered conversation difficult, I certainly felt as if I had put my foot into it, or something like it.

But we are at the bottom of the shaft.

"Stand clear there, glycerine!"

"All right, sir."

"Where's our car?"

"Here, ready; can I help you?"

"Only by keeping clear with your flaring lamps; push on."

And now, impelled by a brakeman, our car is rapidly driven to a small caboose, or cupboard, some three hundred yards from the shaft, the trip relieved by an inquiry:

"How is it the water's so high?"

"A pump gave out last night; water's been gaining since; the machinists will soon fix it."

My companion now unlocked the door of this little caboose on the left side of the tunnel, examined briefly the signal apparatus, an electro-magnet and gong, then the switch or brake, which turns off the current from the wires leading to the heading, and assures himself that whilst charging the drill-holes, no electric spark can pass over the wires by any tampering with the instrument above ground; this done, he resumed the pails, and we now rapidly pushed on to the heading, about one hundred yards distant, the way enlivened by a gushing spring of water; ascended the two benches of rock, we now came upon twelve miners, each with his candle, and the foreman busy examining the finished drill-holes.

"Mr. Gregory, will you send your men back?"

"All hands back from the heading! Glycerine, lads! Pick up your tools; hurry up there, and mind you don't run foul of this man!"

"Where are your holes?"

"Here they are, good and strong."

Eighteen holes are now counted, their diameter and depth gauged; these are found to vary from twenty-six to thirty-two inches in depth, and at various angles, and in various directions from the face, each of them being capable of receiving a cartridge eleven inches long, and one and one-fourth in diameter.

"You need not stay, foreman."

"I'se no fear; I'll just help a bit. Don't mind me; I seen glycerine afore."

Carefully and deliberately a cartridge is removed from the pail; an insulated wire, with priming, exploder, and cork attached thereto, closes the open mouth of the tin cartridge, and still more carefully the cartridge, with its mischievous little wire and fulminating exploder, is now passed into the drill-hole, and pressed down to the extreme end, leaving the wire pendant therefrom like a rat's tail; when this performance has taken place in eighteen holes, a count is made—eighteen.

Now the conducting main wire is brought forward and attached to one of these pendant wires, which, by the way, on close examination, consists of two wires, when attached to one of these, the other is carried to one of the double wires of the next hole, until

each of the eighteen holes is linked with the one next to it, and that to the next, forming a series of links, the first connected with the conducting, the latter with the return wire.

Then two wires, when the switch or break is suitably disposed, connect the cartridges in the holes with the electrical machine, 1,500 feet distant above ground, in the timekceper's office.

Now, bear in mind, there is a break one-tenth of an inch from each other, of the points of the wires in each hole, and this break is armed with a sensitive priming, so that the electric spark, as it leaps from one wire to the other, ignites it; this fires a fulminate, and the explosion of this fulminate explodes the nitro-glycerine, and the nitro-glycerine plays the — with the stubborn, tough, solid rock.

But my mining friend is scrutinizing every connection, and now he counts every hole; none have been missed.

"All back!"

We now turn our backs (with a very satisfactory shrug on my part,) on the masses of rock, burrowed with the eighteen drill-holes, each charged with sufficient nitro-glycerine to hurl it into fragments, aye, from the very bottom of these holes, and to send a blast of liberated gases that will hurl a puff of steam and air out of the shaft 1,500 feet distant.

That pail, I perceive, our companion carries with him. We descend the first bench; at the second he deposits his pail, and we all hurry back to the caboose, where the miner's lights, like the *ignis fatui*, seem right welcome.

But where is there a recess, a safe recess, where I may avoid the consequences of my curiosity? Narrowly watching the miners, I am aroused by the inquiry, sharp and quick in tone:

"All back away from the heading?"

"All back."

"Look out for yourselves!"

And then our sober, decided friend enters the caboose; the door is locked; the miners converse; I endeavor to secure a position by which a good number of miners are between me and that heading, and sit me down on an iron pipe, which, Mr. Gregory informs me, is to supply air to the machine drills.

"Look out, now!"

Instantly, I notice the miners carry their hands to their ears; instinctively I follow suit; the hum of conversation has ceased; a

dead silence succeeds; the pulsation of the steam pump throbs; the breath comes quick;—oh, this suspense—a singular exaltation or excitement thrills through one.

"Boom—oom—ooom!"

A rush of air—my hat has gone with it; pitch dark, for every light and lantern is extinguished.

"Who's got a match?—no one, I bet."

"Yes; here's one."

"A heavy blast, that; she got it that time."

And now the foreman, our companion and myself, make for the heading; the miners are told to keep back.

We return to where the ingenious arrangement of wires, aided by the electric machine, above ground, has effected this discharge.

As we approach within fifty feet of the heading, a warm, sweetish vapor is looming up; still on, on, on: here is a mass of rock; move carefully, there may have been a cartridge thrown out unexploded, laying at your feet. If so, don't trample on it, that's all.

Scrambling over the masses of torn, broken rock, the heading is at last reached—ragged, indented, a scarred witness of the tremendous power of nitro-glycerine.

After carefully noting that each and every hole has been blown out, we return towards the miners. At the second bench, our friend picks up his pail, and assures himself of the safety of the two remaining cartridges.

We soon come to the miners; the word is passed, all safe; another foreman takes in his gang for another eighteen holes, to be drilled in eight hours, the time allotted for each shaft, and pushed back to the shaft, the truck running into the cage.

Signal being given, we commence our ascent—or, better described, now the shaft rushes down, down, down past us.

Daylight once again, and the pleasant warmth of the tunnel is exchanged for the sharp, keen north wind, and 6° below zero temperature. We follow the man with the pails, over the drifting snow, to a shanty, where a good breakfast, hot and glowing fire, await him.

"Breakfast ready, Hoecake?"

"All ready. Blast go off all right, sah?"

"Made two feet heading—hurry up that coffee."

"What do you think of blasting, Mr. —?"

"Well, I think it gives a fellow a sort, of a kind of—new sensasation, decidedly."

BELTING FACTS AND FIGURES NO. II.

By J. H. Cooper.

(Continued from page 326.)

Side of Leather to Pulleys.

"Experience has adduced the fact that when the grain side of a belt is placed next to a pulley, it will drive about 34 per cent. more than when the flesh side is placed next to it."—W. B. in *Sci. Amer.*, March, 1860, p. 150.

"It is pretty generally admitted that leather belts placed with the *grain* side next the pulleys will carry more power; but in connection with this another question arises, namely, *which side placed next the pulley is most durable?*"—*Sci. Amer.*, March, 1860, p. 197.

"Every one knows that the strength of belt leather is on the hair side, and it may be said to lie in about one-fourth the thickness; it is evident then, when that part of the belt is worn away it is no longer of much service."

"I give the flesh side a good coat of tanner's dubbing. I repeat this two or three times in as many days. By this treatment the pores of the leather become filled and the flesh side acquires a smoothness equal to that of the hair side, and my experience has taught me that belts will last six times longer when used in this way than when run on the hair side exclusively."

"This is my experience of twenty years with belts, and when I have treated my leather in the same manner, the same results have always been secured."

"I would be in favor of putting the smooth surface of belts next the pulleys, were it not that they are much more durable when the strong side is kept from wearing."—T. McG., Jr., in *Sci. Amer.*, March, 1860, p. 197.

"The wearing of belts depends altogether upon circumstances. If they adhere well to the pulleys and there is no slipping, but a continual adhesion while at work, leaving the pulleys clear, there is no perceptible wear while running with the hair side to the pulley, but put the rough or flesh side to it, and the wearing of it will soon occur from friction caused by slipping on the pulleys."

"When belts run with the *hair* side next the pulleys, they drive 33 per cent. more machinery and run more steadily than when reversed."—D. I. in *Sci. Amer.*, June, 1860, p. 357.

"Belts rubbed over with neats' foot oil once a week, will give more regular speed, last double the time, and effect a great saving of leather, and experience has established the correctness of placing the smooth side of the belt, next the pulley."—H. M. in *Sci. Amer.*, July, 1848, p. 326.

Influence of the Thickness of Belts.

"When bent round the circumference of a wheel, the outer parts of the belt are distended, the inner parts relaxed; and supposing the section of the belt to be rectangular, the amount of force expended in making these changes is proportional directly to the breadth, to the square of the thickness, and inversely to the diameter of the wheel. Hence if two belts be of like strength, but the one broad and thin, the other narrow and thick, the amounts of force expended in bending them must be proportional directly to their thicknesses, and hence the advantage of using broad thin belts."

"The practice of strengthening belts by riveting on an additional layer must be exceedingly objectionable: indeed, it is difficult to see how any additional strength is gained, for the outer layer must be tight when on the wheel, and slack when free, so that in reality, the strength of only one layer can be available, the parts of the compound belt are puckered and opened alternately, as evinced by the crackling noise."

"The proper procedure is to increase the breadth of the belt."

"So far as we have yet seen, it is preferable to use heavy belts." *Prac. Mech. Journal*, November, 1866, p. 240.

Convexity of Pulleys.

Morin says: "The pulleys over which leather belts pass ought to have a convexity equal to about one-tenth of their breadth."

London *Mech. Mag.*, for March, 1863, says: "belt pulleys should be made slightly convex, in a ratio of half an inch per foot of breadth." Molesworth says the same.

Another proportion is $\frac{1}{8}$ inch wire in 8 inches of width. Still another $\frac{1}{8}$ inch to the foot.

"The rounding should be made as slight as is consistent with security, since every deviation from the cylindric form is accompanied by a loss of force."

"In their progress round the wheels, the different parts of the belt are stretched and relaxed alternately. Now, if the material were perfectly elastic, the force expended on the distension would be reproduced on the contraction of the belt, as the loss by this imperfect elasticity is not known, it will be enough to observe, for the present, that the loss of force will certainly be greater, the greater the disturbance of the particles—the higher the rounding of the pulleys."

Why a Belt runs to the higher part of a Pulley.

Much disputation has been published in efforts to solve the question: Why does a belt run to the higher part of a pulley? There may be several causes, but the chief one is embodied in the following words: That edge of the belt which is towards the larger end of the cone is more rapidly drawn than the other edge, in consequence of this, the advancing part of the belt is thrown in the direction of the larger part of the cone, which obliquity of advance towards the cone must lead the belt on its higher part.

"It may here be observed that this very provision, the rounding of the face of the pulley, which keeps the belt in its place, so long as the machinery is in proper action, tends to throw it off whenever the resistance becomes so great as to cause a slipping."

"To maintain a belt in position on a pulley it is necessary to have the advancing part in the plane of the wheel's rotation."

Running Conditions.

"The slack side on top with large pulleys at high speed, is undoubtedly the true philosophy of transmitting power by belts."

"Speed must not be gained before power in the use of belting. My first experience for myself was running a gang of twelve 7-inch saws and an 8½-inch trimmer, with a 4½-inch pulley."

"I had too much speed and my saws did not run with power to do the work. I then tried a 6-inch pulley, which did better; but still not being satisfied, I had a 7-inch iron one put on, which drove the saws with sufficient power to do all the work."—P. in *Sci. Amer.*, March 1860, p. 150.

"A belt adheres much better and is less liable to slip when at a quick speed than at a slow speed. Therefore, it is much better to gear a mill with small drums, and run them at a high velocity,

than with large drums, and to run them slower; and a mill thus geared costs less and has a much neater appearance, than with large heavy drums; and in belting, if the power of a belt 18 inches wide were required, it would be much better to put in two 9-inch belts than one so wide, owing to the greater inequalities of leather in such large pieces causing loss of adhesion."—I. H. B. in *Frank. Inst. Jour.*, June, 1837, p. 451.

"No belt should be worked up to its full power, and is best run with the slack side on the top and on large pulleys at high velocity, a long slack belt will work for years, but a short one, under heavy strain, is soon destroyed."—R. F. in *Sci. Amer.*, September, 1855, p. 14.

Long belts are preferred to short ones, but care must be taken that the length be not too great.

We have a case in point where a 60-inch pulley at 45 rev. per min. drove a 15-inch pulley, about 50 feet distant, by an 11-inch belt, 109 feet long. The tops of the pulleys were nearly on the same level, and the belt was crossed.

This belt was continually flapping about, soon became crooked and irregular in width and was frequently torn asunder at the lacings by excessive tension, and the whole arrangement proved very troublesome until changed to the following: The speed of the 60-inch, and the diameter of the driven pulley were doubled, and the distance between their centres was reduced to 15 feet. The belt now drives with more power, gives greater regularity of speed and works better every way.

Another case of excessive length, which has come under our notice, is that of an 11-inch open belt on a 4-foot pulley running horizontally at a speed of 2261 feet per minute over a 32-inch pulley, 30½ feet distant. To prevent surging, this belt must be drawn and laced very tightly; too much so for economical running.

Some facts illustrating the evils of short belts were given to me by a friend.

A 30-inch pulley, running 127 rev. per min., drove a 9-inch pulley by a 5-inch belt 14 feet long, the shafts were nearly in a horizontal plane and the lower fold of the belt did the driving.

To do a certain work this belt frequently slipped even when drawn very tightly, so much so, that it tore out at the lacings almost daily, and sometimes three times a day.

After much inconvenience it was changed for a belt 44 feet long;

the 9-inch pulley shaft being removed horizontally to accommodate the increased length, while all the other parts remained the same.

It now performs very satisfactorily; it does not slip, holds at the lacings and the slack fold above sometimes nearly touches the driving one beneath.

The 9-inch pulley shaft carries an 18-inch pulley, which, in its turn, drives a 7-inch pulley below on an Alden fan spindle.

Variation of Speed.

From experiments made, it has been ascertained that about two revolutions per hundred are lost in the transmission of motion by a belt. In ordinary practice this would be a slight loss, and would in no wise interfere with the usual manufacturing processes, but where there is a long train of gear repeated from shaft to shaft by belts, the loss becomes serious.

It is clear if the co-efficient of loss by slippage be ·98 for a single pair, which has been verified with great certainty by varying the tensions of the same belt, it will become equal to the successive powers: ·98, ·96, ·94, ·92, ·90, and so on; so that after a succession of five speeds the loss amounts to $\frac{1}{10}$th of the calculated speed, and that at the end of thirty-four speeds the velocity will be reduced to half.

From these considerations it appears that where it is required to transmit speeds as near determinate as may be, by means of bands and pulleys it is necessary to increase the diameter of the driving pulley by its fiftieth part, or diminish the driven pulley in the same ratio. See *Lond. Mech. Mag.*, March, 1863.

"I have found by experiment that if equal weights were suspended upon opposite sides of the same pulley, by straps of equal wt., but of unequal thickness, the wt. suspended by the thick strap would preponderate, and which seems evident from the consideration that the thick belt carries the wt. further from the centre of motion."—E. M. C. in *Sci. Amer.*, Jan., 1851, p. 142.

"Experiment has taught that ropes, belts, &c., in coiling around cylinders, or pulleys, stretch on the outer side and contract on the inner: the stretch being 2, and the contraction 1, consequently the point that neither stretches nor contracts is $\frac{1}{3}$ the thickness from the inside."—H. W. B. in *Sci. Amer.*, Feb., 1851, p. 158.

"I have always noticed in substituting a thick for a thin belt and *vice-versa*, particularly on machines where the calculations are

nice, that a change in the working of the machine always ensued. Some of our best and most practical manufacturers here, add the thickness of the belt to the diameter of the pulley."—E. B. M., Manchester, N. H., in *Sci. Amer.*, Feb., 1851, p. 174.

Speed.

The best speed for economy is 1000 to 1200 feet per minute, 1500 will do very well, but at 2000 feet per minute economy must be neglected."

"To get the best results from belts they should not be driven more than 1800 feet per minute.

"A leather belt ought to have a velocity of at least 1500 feet per minute, and not more than 2000 feet, or it does not last long.

Comparison of Flat with Round Belts.

The apparatus used for testing the comparative adhesion of good flat and round belting, the latter made under the patent of George Miller, granted in the year 1854, consisted of a horizontal shaft in fixed bearings, carrying a 12-inch pulley of over 3-inch face and a 12-inch wheel with groove $\frac{1}{4}$ inch deep, formed for receiving a $\frac{1}{2}$ inch diameter round belt. Parallel to this shaft and 10 or 12 feet distant was arranged another shaft, with similar pulleys, turning in a sliding frame, carrying also another flat face 12-inch pulley.

A flat 3-inch belt, with *grained* side next the pulleys, on trial would not lift 87 pounds, which were suspended from the periphery of the other 12-inch pulley, a work doubly performed by the $\frac{1}{2}$-inch round belt under the same tension. On the other hand, a tension of 174 pounds on the flat belt, applied to the sliding frame, was necessary to give it sufficient adhesion to lift the 87 pounds weight. It is, therefore, inferred that the round belt possesses an adhesive quality four times that of the flat belt, and must be more durable as the mean increases with the tension.

Experiments prove that $\frac{1}{4}$ inch round belt is more than equal to a 1 inch flat, and a $\frac{1}{2}$ inch round more than a 3-inch flat. The economy of space and material, and diminished friction of journals are points of great importance to manufacturers.

Pulleys for round belts should have a V form not a U-shaped groove.

"These round belts are made by scarfing a broad belt, and rolling it up, not spirally, lengthwise, but in a horizontal fold, so as to

form a perfect round tube, with a very small central bore."—*Sci. Amer.*, April, 1859, p. 285.

Rubber Belting.

"A glazed face is just what I want on a rubber belt; you may be sure you have your belt in good working condition if the face polishes up smooth so as to shine. I have been running rubber belts for fourteen years and have now over 3000 feet of all widths from 2 feet to 2 inches, running in the factory with which I am connected, and I always want to see the faces smooth up and become glossy."

"There is considerable difference in these belts, even in the same lots, but those do best that polish on the face by use, provided they are at the same time flexible."

"I think it is a great mistake to have belts thick; to get power it is much better to add to the width, and never strain the belt too hard; then get your pulleys as smooth as possible, a very little swollen in the middle, say, $\frac{1}{8}$ inch to the foot in width. In starting a rubber belt, the dust should be first brushed off it; if it begins to polish, you may be sure you will have no trouble with it."

"Some of my neighbors use double leather belts which are very expensive; but for economy and keeping up a uniform speed, give me a rubber belt of liberal width, not too thick, but flexible. I have run such belts from three to five years without altering a lacing, and have now running some which have been in use from seven to ten years. There is another good thing in rubber belts; they keep tight on the edges. I have found it a good plan to lag pulleys with a piece of rubber belt; if fitted on neatly it makes a really good lagging."

"At Chickering & Sons' great piano factory, which is close by me, they run with rubber belts; their main belt and pulley are as smooth as glass. There is not in New England, better or neater adjusted machinery than theirs."—B. M. C., Roxbury, Mass., in *Sci. Amer.*, Nov., 1860, p. 294.

"A vulcanized india rubber belt will sustain a greater stress than leather, added to which its resistance to slipping is from 50 to 85 per cent. greater."

(To be continued.)

THE MANUFACTURE AND WEAR OF RAILS.

By Christer Peter Sandberg, Assoc. Inst. C. E.

In these times, when communication between different places is carried on mainly by the system of railways, it becomes important to determine the best mode of manufacturing railway bars, so as to obtain the greatest amount of wear, at the least possible cost.

As this question is one of increasing interest, the author has thought it might be profitable to communicate to the members of the Institution of Civil Engineers, the experience he has gained during the last six or seven years, while engaged in superintending the supply of rails to the three Scandinavian countries, Sweden, Norway and Denmark.

The paper will be divided into three parts. First, as to the best method of manufacturing rails out of common iron, and as to the time they will last. Secondly, as to the disposal of the iron rails when they are worn out; and thirdly, as to whether iron or steel, or a combination of the two materials, is the most economical to use for rails.

The mode of manufacturing rails for Sweden, as carried out in Wales, between the years 1856 to 1860, consisted in hammering the pile for the top slab after the first welding heat, and in rolling it after the second heat. It was supposed that hammering would produce a superior weld and a harder wearing surface, than could be obtained by rolling alone. This method was, however, gradually superseded at other works in England, and in Wales, during the period referred to, by rolling only.

Hammering the slab after the first welding heat entailed an additional charge of 20 per cent. per ton, it therefore became the duty of the Swedish Government to determine, by practical trials, whether the value of the finished rail was correspondingly increased.

With this object in view, several rails were rolled, and arrangements were made for putting them down, in such situations, on some of the English lines, as would expose them to severe wear. The experiments further aimed at discovering, if possible, how long the rails manufactured at the Crown Avon Works, in South Wales, and imported into Sweden, would resist the traffic of that country.

Five different kinds of "piles" were employed, twenty rails, of a

flange section, 4½ inches deep, and weighing 62 pounds per yard, being rolled of each particular sort.

The mode of manufacture was as follows:

The rails marked **T** were made from a pile formed of No. 2, or welded iron, for the top and bottom, the rest of the pile being of No. 1 puddled bar iron. The top slab, and the squares next to it, were made from an hammered bloom of ordinary puddled iron, and filled in at the middle with crop ends, from top slabs and other pieces of No. 2 iron.

The rails marked **Y** were made from a pile of the same composition as that of the **T** rails, with the difference that the pile for the top slab consisted of puddled bars, without any welded iron pieces or crop ends being introduced in the middle of the pile.

The pile for the rails marked **H** was composed of a top slab made from puddled bars, hammered after the first heat, and rolled after the second heat, similar to the rails marked **Y**, the iron for the flange consisting of four pieces instead of eight.

The pile for the rails marked **E** was exactly similar to that for rails marked **H**, excepting that the pile for the top slabs was rolled after the first heat, as well as after the second heat. This difference in the mode of manufacture was adopted in order to discover whether, in this common iron, hammering improved the rail to a corresponding extent. Instead of No. 2 iron, puddled bars were chiefly used for the squares near the slab, and for the foot of the rail.

The pile for the rails marked **N** consisted of puddled bars, without any top slab.

All the rails passed through the rolling successfully, with the exception of the **N** rails, some of which showed cracks, owing to the inferior quality of the puddled bar.

The London and Northwestern Railway Company, being interested in this problem, allowed experiments to be made on their line, so far as the wear of these experimental rails was concerned. The experiments were carried out at Camden Town Station, where the rails could be better and more thoroughly tried than elsewhere; first, on account of the enormous traffic which obtains at that spot; secondly, from the constant shunting; and thirdly, owing to the grinding action of the engine-wheels in starting the trains. The result of these experiments is shown in a series of tables, drawn up

by Mr. H. Woodhouse, Superintendent of Permanent Way.—*See Appendix.*

The following Tables show the number of tons passed over each experimental rail before it was crushed, and also before it was taken out.

Mark of Rails.	Crushed.	Worn Out.
	Tons.	Tons.
T	3,680,000	5,060,000
Y	4,140,000	5,290,000
H	3,220,000	5,000,000
E	6,900,000	8,970,000
N	3,220,000	5,520,000

Another table, calculated from the preceding one, shows how long the rails will last, supposing them to be passed over by 3,000 trains yearly, each train being composed of an engine weighing 30 tons, and of 20 wagons of 10 tons each, or a gross load of 230 tons.

From these Tables it was ascertained that the five different descriptions of rails were, on the average, crushed in 6 years, and worn out in 9 years, thus:

T		Y		H		E		N	
Crushed.	Worn Out.	Crushed.	Worn Out.	Crushed.	Worn Out.	Crushed.	Worn Out.	Crushed.	Worn Out.
Years.	Years.	Years.	Years.	Years.	Years.	Years.	Years.	Years.	Years.
5	7	6	7	5	7	10	13	5	8

As the object of these experiments was chiefly to ascertain the difference between a rolled and a hammered slab, both made from inferior iron, **E** representing the former, and **H** the latter, those rails were placed so as to compare their relative resistance to wear

and the result shows the **E** rail, with the rolled slab, to be superior at each place where the rails were tested.

Among the other descriptions of rails, the **N** section endured the longest, although it had no top slab of No. 2 iron.

The conclusion is thus arrived at, that hammering after the first welding heat, for this particular kind of iron, does not improve the endurance of the rails, but that the simplest mode of manufacture has also the material advantage of being the best. These trials, at the same time, establish the fact, that it is not the wear or diminished sectional area, caused by abrasion, which produces the unsatisfactory results in the endurance of iron rails, but the lamination caused by imperfect welding. This explains the great difference in the result between the wear of rails made in exactly the same way, the welding in the one case being perfect, whilst in the other it has been very imperfect.

The results obtained at the Camden Town Station, however, are not applicable to the circumstances and conditions of the wear of rails which occurs under ordinary traffic, but rather to exceptionable situations, where the wear is occasioned, principally, by the frequent use of the breaks, and by continual shunting, in a much higher degree than at any other point of the line. These results may also be attributed, in part, to the great weight of the locomotives, in proportion to the weight of this particular section of rail.

Rails of the same dimensions, and of similar quality of iron to those marked **E**, have been tried on the Great Northern Railway, and have lasted during the passage of about 65,000 trains, of a total aggregate weight of 13,000,000 tons, one-fourth part of this traffic being at a speed of about forty English miles per hour, and the remaining three-fourths of fifteen miles an hour.

These experiments confirm the rule laid down in Mr. R. Price Williams's paper "On the Maintenance of Permanent Way," viz: that the endurance of rails may be measured by the product of the speed and of the passing weight. The trial rails on the Great Northern Railway may thus be said to have borne 276,000,000 tons, at a speed of one mile per hour. The endurance of the rails tried at Camden Town, under such unusual circumstances, is much less, and may be represented by 120,000,000 tons at a speed of one mile per hour.

Another mode of arriving at a judgment as to the endurance of these rails on the Swedish State Lines, is found in the renewals

Piles for Iron Rails.
Swedish Rail section.
T. & Y.
H. & E.
N
Iron No 2 or Welded iron.
Puddle bars
Bessemer Steel
62 lbs per Yard
Pile for the top slab for the Rails T.& Y., H.& E.
Piles for Steel Headed Rails.
Bessemer Steel slab.
Bessemer Steel.
Swedish Rail Section
66 lbs per Yard.
Swedish Rail Section
66 lbs per Yard.
85 lbs per Yard
Plan A.
Plan B.
Plan C.

which have been already made on those railways. The present experience extends over a period of nine years on portions of the most severely worked lines, namely, at Gottenburg and Malmo; and also during a period of six years on the whole system. The renewals on the main line have been in an increasing proportion, being in one case 30 or 40 per cent. higher than in the preceding year; and a mean calculation gives the probable result that, taking the last renewals of the rails laid down on the lines at about eighteen years from the commencement, the average life of the rails will be fifteen years.

The weight which has passed over the rails during these fifteen years, judging from the reports contained in the annual accounts of the Government Railway Administration, as to the traffic returns of all the State lines, during the years 1862 to 1865, may be assumed to be a yearly increase, in the gross load, of only 15 per cent. per mile, after the year 1865. The yearly increase, however, on the line nearest Gottenburg, since 1862, of transported goods, has amounted to 30 per cent., and near Malmo to 18 per cent. Further, supposing the same proportion to exist between the gross and the nett loads for the year 1865, it may be taken at about 6,000,000 tons, a quantity which, compared with the results obtained on the English lines, giving for the life of the best of the three first sorts of rails, the same average life as that of the **E** rails. This confirms the correctness of the theory that the life of rails is measured by the product of the weight and the speed.

The rails used on the Swedish lines are mostly of the **E** or rolled class before mentioned. Those tried on the Great Northern line were also of that kind of manufacture, but of a heavier section. The speed at which the load was carried over the experimental rails on the Great Northern Railway was much higher than on the Swedish lines, being in the proportion of twenty on the Great Northern to sixteen miles average speed on the Swedish railways.

The conclusions the author has arrived at are, that no rule can be laid down for the manufacture of rails that will apply to every manufacturing district; but, that in the case of Welsh iron, to which he has more particularly referred, it has been proved that the best mode of manufacturing the rails is that now most commonly practiced, viz: rolling the iron into bars, piling these, and repeated rolling to the finished rail, without hammering. The author assumes the prejudicial result from hammering to be owing

to the large amount of sulphur in the Welsh iron. Where the iron contains more phosphorus and less sulphur, as, for instance, in the Cleveland, Belgian, and French iron districts, hammering has proved beneficial, and rails have been made direct from puddled bars, without the intermediate process of piling, this being, in fact, the method generally adopted in those places, and being found to answer best.

These experiments seem to indicate that 220,000,000 tons may be carried over rails, of the section and make referred to, at a speed of one mile per hour; so that any railway company, knowing the load which yearly passes over their line, and the speed, may, by multiplying the one into the other, and dividing that product by 220, ascertain the life of iron rails in years.

(To be continued.)

THE ECONOMICAL CONSTRUCTION OF BEAM TRUSSES.

By G. S. Morison, C. E.

(Continued from page 310.)

Trusses continuous through a number of Spans.

If a single loaded beam rests upon several supports, the load will cause it to bend in a manner similar to that shown in Fig. 31 (Plate II), the curvature being reversed once in each end span, and twice in every intermediate span. The parabolas denoting the bending strains will form a series similar to that shown in the same figure. As the bending strains vanish at the points of reversal, the intermediate part of the beam becomes for the time an independent girder; one-half of its weight is carried in either direction, and the sign of the shearing strains changes midway between these points of reversal, instead of at the centre of the beam, as in isolated spans. But though both classes of strains are affected materially by the condition of continuity, the laws which govern their relative variations do not change; the shearing strains increase uniformly while the load is uniform, and the increment of the chord strains is everywhere the shearing strain divided by the depth of truss. If the negative bending strains over the piers are once known, all other strains are easily determined.

Let A B C D, (Fig. 32, Plate III.), represent a part of a beam distorted by a load, and A *b c* D the same when relieved of its strains. Unless

the distortion is excessive, A B may be regarded equal to A E and D F to D c, the elongation of the lower chord being F C, and the shortening of the upper chord, E B. The amount of curvature is given by the angle, G S T = E *o b* = F *o* C; the two last angles are respectively proportional to E B and F C, which, being the changes of length caused by the chord strains, are proportional to the sum of the strains acting in A B and D C. It follows that the amount of curvature in a loaded beam is everywhere proportional to the bending or chord strain.

In Fig. 33, Plate III., A B represents one span of a continuous girder. Let S_1 denote the negative chord strain at A and S_2 that at B, A_1 the shearing strain at A and B, that at B, S the chord strain at any point, P, distant x from A. The shearing strain at P will be—

$$A_1 - wx$$

the sum of the shearing strains between A and P

$$A_1 x - \frac{wx^2}{2}$$

the chord strain at P

$$S = S_1 + \frac{A_1 x}{h} - \frac{wx^2}{2h} \qquad (1.)$$

and the sum of the chord strain between A and P, given by the negative and positive areas F A M and M X P, or F M X L — A P L F

$$S_1 x + \frac{A_1 x^2}{2h} - \frac{wx^3}{6h}$$

Since the amount of curvature is proportional to this sum, angle

$$O T U = \text{angle } O P Q + \text{angle } U R S = c\left(S_1 x + \frac{A_1 x^2}{2h} - \frac{wx^3}{6h}\right) \qquad (2.)$$

c being a constant determined by the depth of truss, nature of its material, &c.

Representing by y the elevation or depression of the point P above or below A, tang. angle O P Q becomes the differential co-efficient of y taken relatively to x; as O P Q is exceedingly small, the angle itself may be taken equal to its tangent, and the integral of the last equation gives

$$y - x \times \text{angle } U R S = c\left(\frac{S_1 x^2}{2} + \frac{A_1 x^3}{6h} - \frac{wx^4}{24h}\right) \qquad (3.)$$

eliminating A between equations (1) and (3.)

$$y - x \times \text{angle } URS = c\left(\frac{S_1 x^2}{2} + \frac{S x^2}{6} + \frac{wx^4}{24h}\right) \qquad (4.)$$

For the point B, if the two supports are of equal height,

$x = l$, $y = o$, U R S = V X Z, and S = s^2, and equation (4) becomes—

$$\text{angle V X Z} = c\left(\frac{s_1 l}{3} + \frac{s_2 l}{6} + \frac{w l^3}{24 h}\right) \qquad (5.)$$

Applying equation (5) to Fig. 30, and denoting the strains over the three supports A B and C by s_1, s_2, and s_3, the lengths A B and B C, by l and l', and their respective loads by w and w', we hence—

$$\text{angle R S T} = c\left(\frac{s_2 l'}{3} + \frac{s_3 l'}{6} + \frac{w' l'^3}{24 h}\right)$$

$$\text{angle P S U} = c\left(\frac{s_2 l}{3} + \frac{s_1 l}{6} + \frac{w l^3}{24 h}\right)$$

But angle P S U = — angle R S T, have—

$$\frac{s_2 l'}{3} + \frac{s_3 l'}{6} + \frac{w' l'^3}{24 h} = -\frac{s_2 l}{3} - \frac{s_1 l}{6} + \frac{w l^3}{24 h}$$

and reducing—

$$s_1 l + 2 s_2 (l + l') + s_3 l' + \frac{w l^3}{4 h} + \frac{w' l'^3}{4 h} = 0$$

an equation expressing a single relation between the strains over these successive points of support.

Applying this equation to each set of three successive supports, two less equations are obtained than the entire number of supports. But since the strains over the extreme supports at each end of the girder are equal to zero, the number of equations is the same as the number of unknown quantities.

The shearing strains are determined by equation (1.) which gives, x being made equal to l—

$$A_1 = \frac{(s_2 - s_1) h}{l} + \frac{w l}{2}$$

and throughout the span these strains will differ from those in a similar isolated span by the constant quantity, $\frac{(s_2 - s_1) h}{l}$. They are more intense at one end than in an isolated spar, and less so at the other, the greater intensity being at that end at which the negative bending strain is the greater.

Having determined the chord strains over the piers, and the shearing strains for the corresponding load, the chord strains at any point may be readily calculated, but will be more readily found by drawing the parabola indicating them. This parabola passes through the points F and G (Fig. 33), A F being equal to s_2 and B G to s_2, and has for its axis the line O H, perpendicular to A B at the point where the shearing strain changes its sign. Since the curvature of this parabola is determined by the inclination of the line, D E, which is de-

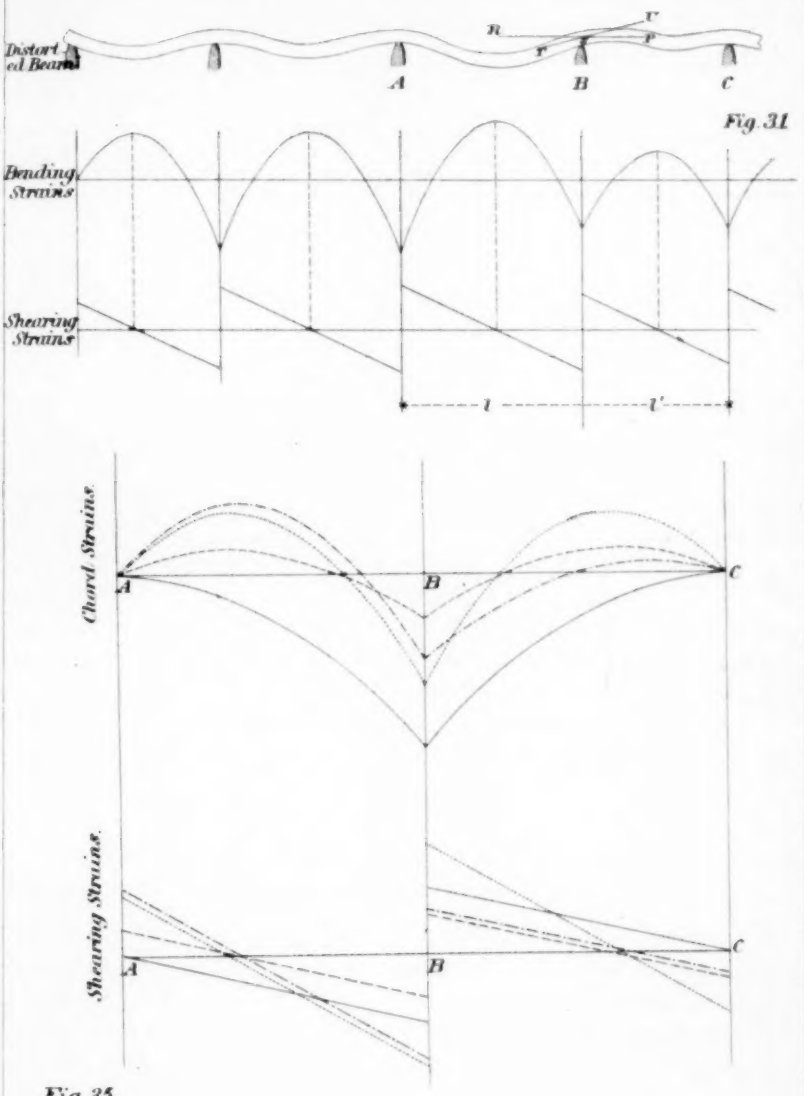


Fig 35.

Neither Span Loaded.

Both Spans Loaded.

One Span Loaded.

Bridge Swung.

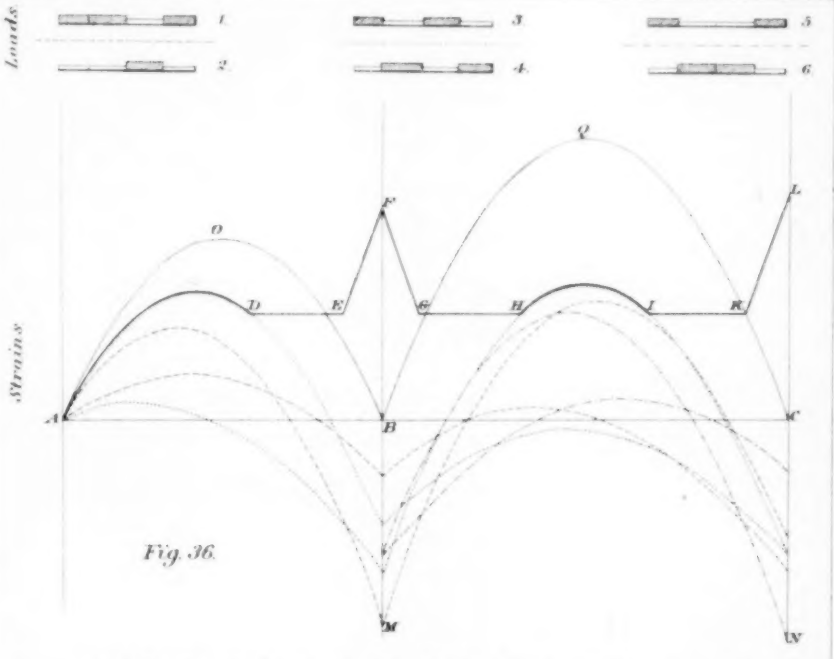


Fig. 36.

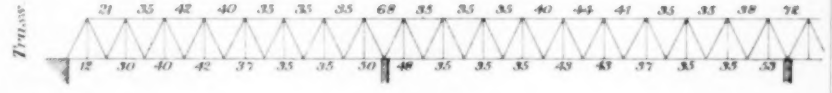
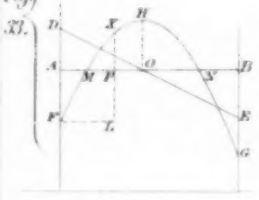


Fig. XI.



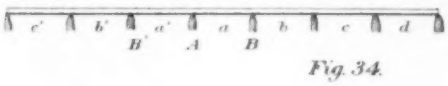


Fig. 34.

Fig. 32.

pendent solely on the intensity of the load, the parabola will have the same parameter as in a disconnected span, or in a span of different length, but loaded with the same weight per foot.

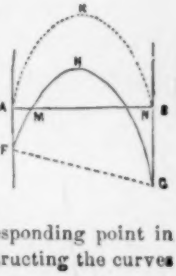
The strains in each span differ materially with the distribution of the load upon the other spans, and it is necessary to consider a number of arrangements of this load, and to proportion the chords at each point with reference to the maximum strain under any possible arrangement. This, though a matter of considerable difficulty when the strains are all calculated, becomes exceedingly simple when they are determined by constructing the corresponding curves.*

It is not necessary to consider all possible distributions of the load, but in general it will be enough to treat each successive span as loaded, and then examine three distributions which make the strains most intense at each end, and at the centre of this span, the last condition occurring when the sum of the strains at both ends of the span is least. When great accuracy is required, the distributions complementary to these should also be examined, though as this will affect the results only near the points of contrary flexure where a considerable excess of metal will always be placed, such an examination is seldom necessary.

Referring to Fig. 34, Plate III., it appears that a weight on either of the spans, a or a^1 increases the strain over A, while weights on b and b^1 by balancing in part the weights on a and a^1, relieves the strain at A_1; and again a load on c and c_1 will relieve the strains at B and B_1,

* The easiest method of drawing these parabolas is to cut a parabola of the desired parameter from a piece of stiff card board, and keeping the axis vertical, move it about till it passes through the points representing s_1 and s_2, then by drawing the pencil along the edge of the card, the desired curve is obtained. The same course should then be repeated for the values of s_1 and s_2, corresponding to a different distribution of the load. If the case of the unloaded span is also considered, two patterns will be needed, one corresponding to the intensity of the dead load, and the other to the moving load alone.

From equations (1) and (6) we obtain for the general equation of the curve of the chord strains (F H G), the origin being transferred to the centre of the span, $s = \frac{s_1(\frac{1}{2}l - x) + s_2(\frac{1}{2}l + x)}{l} + \frac{wl^2 - 2_1x^2}{8h}$, the equation of the corresponding curve (A R B) for an isolated span being $s = \frac{wl^2 - 4x^2}{8h}$ and since $y = s_1\frac{(\frac{1}{2}l - x) + s_2\frac{1}{2}l + x)}{x}$ is the equation of the line F G, the distance of any point in F H G from the line $F G_1$ is equal to the ordinate of the corresponding point in A R B. This condition furnishes a second simple method of constructing the curves of the chord strain in a continuous girder.



and increase that at A; and in general, the strain over a pier is increased by a weight on the first, third, fifth, &c. span on either side of it, and diminished by a weight on the second, fourth, sixth, &c. The influence of a weight on the spans, b and c is greater at B than at the more distant support, A, and the sum of the strains at A and B will therefore be increased by weights on b a^1 d c^1, &c., and diminished by weights on c b^1, &c.; hence the strain at the centre of a span is diminished by a load on the first, third, fifth, &c., span on either side of it, and increased by a load on the second, fourth, sixth, &c.

As in each end span there is but one point of contrary flexure, the strains in the end spans will be wholly out of proportion to those in the intermediate spans, unless there be a proper difference in their lengths. In a beam fastened at the ends, the distance from each point of reversal to the nearer support has been found to be $\cdot 2113\ l$; in a continuous girder of two equal loaded spans, this distance is $\cdot 25\ l$; and it will be found to average about one-fifth the length of an intermediate, and one-fourth that of an end span. By making the length of the end spans of a continuous truss four-fifths that of the intermediate spans, very nearly the true proportion is obtained, and this simple ratio will be consistent with the use of panels of equal length in every span of a bridge.

The saving of material effected by building a bridge in continuous instead of isolated spans, though obviously great, depends too much upon the special circumstances governing each case, to be expressed by a formula or stated in a table. It is relatively greater in long than in short spans, owing to the increased dead load, greater in a bridge of a large number of spans, than in one of only two or three, while its full advantages will only be obtained when the proper ratio between the lengths of the end and intermediate spans is adopted. An examination of two simple cases will illustrate the use of the formulæ deduced above, and also indicate the economical character of a continuous girder. The first case examined will be that of a pivot draw, the most common example of two connected spans; and the second that of a bridge of four continuous fixed spans.

Case of a Pivot Draw.

The two spans are of equal length; the strains may be examined under four arrangements, including that in which the draw is

swung. When closed, the bridge rests upon three supports, and we have—

$l = l'$ $\quad s_1 = 0 \quad s_3 = 0$ and equation (6.) becomes—

$$4l s^2 + \frac{w_1 l^3}{4h} + \frac{w_2 l^3}{4h} = 0$$

$$s^2 = -\frac{l^2}{16h}(w_1 + w_2)$$

Equation (7.) gives for the shearing strains at the first and third supports—

$$A_1 = \frac{(7w_1 - w_2)l}{16} \qquad C_2 = -\frac{(7w_2 - w_1)l}{16}$$

When the bridge is swung, it becomes a beam resting upon one central support; the shearing strains on either side of this support will be equal to the dead load of one arm of the draw, or $\pm wl$ (w denoting the dead, and w^1 the moving load), decreasing uniformly towards the ends where they vanish. The chord strains are always negative, and vary as the external ordinates of a parabola, from 0 at the ends to $-\frac{wl^2}{2h}$ over the central support. Under the following circumstances, the strains at the support are—

Neither span loaded,	$s_2 = -\frac{l^2}{8h}w$	$A_1 = \frac{3l}{8}w$	$C_2 = -\frac{3l}{8}w$
First " "	$s_2 = -\frac{l^2}{16h}(2w + w^1)$	$A_1 = \frac{l}{16}(6w + 7w^1)$	$C_2 = -\frac{l}{16}(6w - w^1)$
Both " "	$s_2 = -\frac{l^2}{8h}(w + w^1)$	$A_1 = \frac{3l}{8}(w + w^1)$	$C_2 = -\frac{3l}{8}(w + w^1)$
Bridge swung,	$s_2 = -\frac{l^2}{2h}w$	$A_1 = C_2 = 0$	

The curves indicating the strains through the two entire spans, are shown in Fig. 35 (Plate III.), w being taken equal to $\frac{2}{3}w^1$. When $\frac{l^2}{2h}w > -\frac{l^2}{8h}(w + w^1)$, or $w > -\frac{w^1}{3}$ which will almost always be the case, but two cases need be considered in proportioning the chords that of the open draw, and that of the single loaded span.

It is to be observed, however, that these results are correct only when the draw is fitted with a latch capable of throwing the proper proportion of the dead weight upon the rest piers; otherwise the entire dead weight may always be borne by the centre pier, and the moving load alone should then be distributed in the manner considered above.

(To be continued.)

Mechanics, Physics, and Chemistry.

LECTURE-NOTES ON PHYSICS.

By Prof. Alfred M. Mayer, Ph.D.

(Concluded from page 343).

"This first result established, let us recur to the general expression of the pressure corresponding to any point of a liquid surface, an expression which is $P + \frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$. For a surface of convex spherical curvature, if we designate by d the diameter of the sphere to which this surface pertains, the above expression becomes $P+\frac{2A}{d}$, and for a spherical surface of concave curvature pertaining to a sphere of the same diameter, we shall have $P-\frac{2A}{d}$. Thus, in the case of the convex surface the total pressure is the sum of two forces acting in the same direction—force, of which one designated by P is the pressure which a plane surface would exert, and the other represented by $\frac{2A}{d}$ is the action which depends on the curvature. On the contrary, in the case of the concave surface the total pressure is the difference between two forces acting in opposite directions, and which are again, one the action P of a plane surface, and the other $\frac{2A}{d}$, which depends on the curvature. Whence it is seen that the quantity $\frac{4A}{d}$, which represents the pressure exerted by a spherical film on the air it encloses, is equal to double the action which proceeds from the curvature of one or the other surface of the film.

"Now, when a liquid rises in a capillary tube, and the diameter of this is sufficiently small, we know that the surface which terminates the column raised does not differ sensibly from a concave hemisphere, whose diameter is consequently equal to that of the tube. Let us recall, moreover, a part of the reasoning by which we arrive, in the theory of capillary action, at the law which connects the

height of the column raised with the diameter of the tube. Let us suppose a pipe, excessively slender, proceeding from the lowest point of the hemispheric surface in question, descending vertically to the lower orifice of the tube, then bending horizontally, and finally rising again so as to terminate vertically at a point of the plane surface of the liquid exterior to the tube. The pressures corresponding to the two orifices of this little pipe will be, on the one part, P, and on the other, $P - \frac{2A}{\delta}$, if by δ be designated the diameter of the concave hemisphere, or, what amounts to the same thing, that of the tube. Now, the two forces P, mutually destroying one another, there remains only the force $-\frac{2A}{\delta}$, which, having a sign contrary to that of P, acts consequently from below upwards at the lower point of the concave hemisphere, and it is this which sustains the weight of the molecular thread contained in the first branch of the little pipe between the point just mentioned and a point situated at the height of the exterior level. This premised, let us remark that the quantity $\frac{2A}{\delta}$ is the action which results from the curvature of the concave surface. The double of this quantity or $\frac{4A}{\delta}$, will therefore express the pressure exerted on the enclosed air by a laminar sphere or hollow bubble of the diameter δ, and formed of the same liquid. It thence results that this pressure constitutes a force capable of sustaining the liquid at a height double that to which it rises in the capillary tube, and that, consequently, it would form an equilibrium to the pressure of a column of the same liquid having that double height. Let us suppose, for the sake of precision, δ equal to a millimètre, and designate by h the height at which the liquid stops in a tube of that diameter. We shall have this new result, that the pressure exerted on the enclosed air by a hollow bubble formed of a given liquid and having a diameter of 1 millimètre, would form an equilibrium to that exerted by a column of this liquid of a height equal to $2h$. Now, the pressure exerted by a bubble being in inverse ratio to the diameter thereof, it follows that the liquid column which would form an equilibrium to the pressure exerted by a bubble of any diameter whatever, d, will have a height equal to $\frac{2h}{d}$.

"It would seem, at first, that this last expression ought to apply equally well to liquids which sink in capillary tubes, h then designating this subsidence, the tube still being supposed 1 millimètre in diameter; but is not altogether so, for that would require, as is readily seen by the reasonings which precede, that the surface which terminates the depressed column in the capillary tube should be sensibly a convex hemisphere; now we know that in the case of mercury this surface is less curved; according to the observations of M. Bède, its height is but about half of the radius of the tube; whence it follows that the valuation of the pressure yielded by our formula would be too small in regard to such liquids. It may be considered, however, as a first approximation.

Let us take, as a measure of the pressure exerted by a bubble, the height of the column of water to which it would form an equilibrium. Then, if δ designates the density of the liquid of which the bubble is formed, that of water being 1, the heights of the columns of water and of the liquid in question which would form an equilibrium to the same pressure will be to one another, in the inverse ratio of the densities, and, therefore, if the height of the second is $\frac{2h}{d}$, that of the first will be $\frac{2h\delta}{d}$. Hence, designating by p the pressure exerted by a laminar sphere on the air which it encloses, we obtain definitely $p=\frac{2h\delta}{d}$, δ being, as we have seen, the density of the liquid which constitutes the film, h the height to which this liquid rises in a capillary tube 1 millimètre in diameter, and d the diameter of the bubble. If, for example, the bubble be formed of pure water, we have $\delta=1$, and, according to the measurements taken by physicists, we have, very exactly, $h=30^{mm}$; the above formula, therefore, will give, in this case, $p=\frac{60}{d}$. If we could form a bubble of pure water of one decimetre or 100^{mm}, in diameter, the pressure which it would exert would consequently be equal to $0^{mm}{\cdot}6$, or, in other terms, would form an equilibrium to the pressure of a column of water $0^{mm}{\cdot}6$ in height; the pressure exerted by a bubble of the same liquid one centimetre, or 10^{mm} in diameter, would form an equilibrium to that of a column of water 6^{mm}. As regards soap-bubbles, their pressures, if the solution were as weak as possible, would differ very little from

those exerted by bubbles of the same diameters formed of pure water.

"For mercury we have $\delta = 13{\cdot}59$, and, according to M. Bède, h about equal to 10^{mm}; the formula would therefore give, for a bubble of mercury $p = \frac{271{\cdot}8}{d}$, but, from the remark which closes the last paragraph, this value is too weak, and can only be regarded as a first approximation. It only instructs us that, with an equality of diameter, the pressure of a bubble of mercury would exceed four and a half times that of a bubble of pure water. For sulphuric ether, we have $\delta = 0{\cdot}715$, and conclude from measurements taken from M. Frankenheim, h to be very closely to $10^{mm}{\cdot}2$; whence results $p = \frac{14{\cdot}6}{d}$, and thus, with an equal diameter, the pressure of a bubble of sulphuric ether would be but the fourth of that of a bubble of pure water.

We know that the product $h\delta$ being the product of the capillary height by the density, is proportional to the molecular attraction of the liquid for itself, or, in other terms, to the cohesion of the liquid; (see research of Prof. Henry on Cohesion of Liquids, quoted in §V.) it is, moreover, the result from a comparison of the values $\frac{4A}{d}$ and $\frac{2h\delta}{d}$, which have been found to represent the pressure exerted by a lan inar sphere on the air which it contains; hence we deduce $h\delta = 2A$, and it will be remembered that A is the capillary constant; that is to say, a quantity proportional to the cohesion of the liquid. The formula $p = \frac{2h\delta}{d}$ indicates, therefore, as must be evident, that the pressure exerted by a laminary bubble on the included air is in the direct ratio of the cohesion of the liquid which constitutes the film and the inverse ratio of the diameter of the bubble.

"As early as 1830, a learned American, Dr. Hough, had sought to arrive at the measure of pressure exerted, whether on a bubble of air contained in an indefinite liquid or on the air enclosed in a bubble of soap.

(*Inquiries into the principles of liquid attraction.*—Silliman's Journal, 1st series, vol. xvii., page 86.)

"He conceives quite a just idea of the cause of these pressures which he does not, however, distinguish from one another, and, in

order to appreciate them, sets out, as I have done, with a consideration of the concave surface which terminates a column of the same liquid raised in a capillary tube; but, although an ingenious observer, he was deficient in a knowledge of the theory of capillary action, and hence arrives, by reasoning, of which the error is palpable, at values and a law which are necessarily false.

"Prof. Henry, in a very remarkable verbal communication on the cohesion of liquids, made in 1844, to the American Philosophical Society, (Philosophical Magazine, 1845, vol. xxvi., page 541), described experiments by means of which he had sought to measure the pressure exerted on the internal air by a bubble of soap of a given diameter. According to the account rendered of this communication, the mode of operation adopted by Mr. Henry was essentially as follows: he availed himself of a glass tube of **U** form, of small interior diameter, one of whose branches was bell-shaped at its extremity, and inflated a soap-bubble extending to the edge of this widened portion; he then introduced into the tube a certain quantity of water, and the difference of level in the two branches gave him the measure of the pressure. Unfortunately, the statement given does not make known the numbers obtained, nor does it appear that Mr. Henry has subsequently published them. This physicist refers the phenomenon to its real cause, and states the law which connects the pressure with the diameter of the bubble; the account does not say whether the experiments verified it. But Mr. Henry considers that a hollow bubble may be assimilated to a full sphere reduced to its compressing surface; that is to say, he attributes the phenomenon to the action of the exterior surface of the bubble, without taking into account that of the interior surface. Let us add that, in the same communication, Mr. Henry has mentioned several experiments which he had made on the films of soap and water, and which, from the statement given would elucidate, in a remarkable manner, the principles of the theory of capillary action. It is much to be regretted that these experiments are not described

"In a memoir presented to the Philomatic Society in 1856, and printed in 1859 in the *Comptes Rendus*, (tome xlviii., page 1405.) M. de Tessan maintains that if the vapor which forms clouds and fogs were composed of vesicles, the air enclosed in a vesicle of 0·02 millimètre diameter would be subjected, on the part of this vesicle, to a pressure equivalent to ¼ of an atmosphere. M. de Tessan does

not say in what manner he obtained this valuation; but it is easily seen that he has fallen into an error analogous to that of Professor Henry, in the sense that he pays no attention except to the exterior surface of the liquid pellicle. According to the formula of the preceding paragraph, the pressure exerted on the interior air by a bubble of water of 0·02 millimètre diameter would, in fact, be equivalent to that of a column of water 3 metres in height, which equals nearly $\frac{3}{4}$ of the atmospheric pressure; M. de Tessan has found then but half the real value, and we know that this half is the action due to the curvature of one only of the surfaces of the film.

"After having obtained the general expression of the pressure exerted by a laminar sphere on the air which it encloses, it remained for me to submit my formula to the control of experiment. I have employed, with that view, the process of Mr. Henry, which means that the pressure was directly measured by the height of the column of water to which it formed an equilibrium.

From our formula we deduce $p\,d = 2\,h\,\varsigma$; for the same liquid, and at the same temperature, the product of the pressure by the diameter of the bubble must, therefore, be constant, since h and ς are so. It is this constancy which I have first sought to verify for bubbles of glyceric liquid—(a solution of Marseilles soap and glycerine in distilled water, with which Plateau made his bubbles)—of different diameters."

Plateau here describes his apparatus and the precautions to be used in making the measures, which the reader will find detailed in the Smithsonian Report for 1865.

"The following table contains the results of these experiments; I have arranged them, not in the order in which they were obtained, but in the ascending order of the diameters, and I have distributed them into groups of analogous diameters. During the continuance of the operations the temperature varied from 18°·5 to 20° C.

* * * * As the first diameter is to those of the last group very nearly as 1 to 6, these results suffice, I think, to establish distinctly the constancy of the product $p\,d$, and consequently the law according to which the pressure is in the inverse ratio of the diameter.

* * * * * * * *

"As to the general mean 22·75 of the results of the table, its decimal part is necessarily a little too high, on account of the excessive value 26·45 of the last product. As this product, and that which

precedes it, are those which alone deviate materially from 22 in this integral part, it will be admitted, I think, that a nearer approach to the true value will be made by neglecting these two products and taking the mean of the others, a mean which is 22·56, or more simply 22·6; we shall adopt, then, this last number for the value of the product $p\,d$ in regard to the glyceric liquid.

Diameters, or values of d.	Pressures, or values of p.	Products, or values of $p\,d$.
m m	m m	
7·55	3·00	22·65
10·37	2·17	22·50
10·55	2·13	22·47
23·35	0·98	22·88
26·44	0·83	21·94
27·58	0·83	22·89
46·60	0·48	22·37
47·47	0·48	22·78
47·85	0·43	20·57
48·10	0·55	26·45

It remained to be verified whether this value satisfied our formula, according to which we have $p\,d = 2\,h\,\varsigma$, the quantities ς and h being respectively, as we have seen, the density of the liquid and the height which this liquid would attain in a capillary tube 1 millimètre in diameter. With this view, therefore, it was necessary to seek the values of these two quantities in reference to the glyceric liquid. The density was determined by means of the aerometer of Fahrenheit, at the temperature of 17° C., a temperature little inferior to that of the preceding experiments, and the result was $\varsigma = 1{\cdot}1065$. To determine the capillary height the process of Gay Lussac was employed, that is, the measurement by the cathetometer, all known precautions being taken to secure an exact result. The experiment was made at the temperature of 19° C. * * * * * * * The reading of the cathetometer gave, for the distance from the lowest point of the concave meniscus to the exterior level, $27^{m\,m}{\cdot}35$.

This measurement having been taken, the tube was removed, cut at the point reached by the capillary column, and its interior diameter at that point measured by means of a microscope, furnished

with a micrometer, giving directly hundredths of a millimètre. It was found that the interior section of the tube was slightly elliptical, the greater diameter being $0^{mm}{\cdot}374$ and the smaller $0^{mm}{\cdot}357$; the mean was adopted, namely, $0^{mm}{\cdot}3655$, to represent the interior diameter of the tube assumed to be cylindrical. To have the true height of the capillary column, it is necessary, we know, to add to the height of the lowest point of the meniscus the sixth part of the diameter of the tube, or, in the present case, $0^{mm}{\cdot}06$; the true height of our column is consequently $27^{mm}{\cdot}41$. Now, to obtain the height h to which the same liquid would rise in a tube having an interior diameter of exactly a millimètre, it is sufficient, in virtue of the known law, to multiply the above height by the diameter of the tube, and thus we find definitively $h = 10^{mm}{\cdot}018$.

"I should here say for what reason I have chosen for the experiment a tube whose interior diameter is considerably less than a millimètre. The reasoning by which I arrived at the formula supposes that the surface which terminates the capillary column is hemispherical; now that is not strictly true, but in a tube so narrow as that which I have employed, the difference is wholly inapplicable, so that in afterwards calculating, by the law of the inverse ratio of the elevation to the diameter, the height for a tube one millimètre in diameter, we would have this height such as it would be if the upper surface were exactly hemispherical.

"The values of ϖ and h being thus determined, we deduce therefrom $2\,h\,\varpi = 22{\cdot}17$, a number which differs but little from 22·56 obtained above as the value of the product $p\,d$. The formula $p\,d = 2\,h\,\varpi$ may therefore be regarded as verified by experiment, and the verification will appear still more complete if we consider that the two results are respectively deduced from elements altogether different. I hope hereafter to obtain new verifications with other liquids.

Investigation of a very small limit below which is found, in the glyceric liquid, the value of the radius of sensible activity of the molecular attraction.

"The exactness of the formula $p = \frac{2\,h\,\varpi}{d}$ supposes, as we are about to show, that the film which constitutes the bubble has, at all points, no thickness less than double the radius of sensible activity of the molecular attraction.

"We have seen that the pressure exerted by a bubble on the air which it encloses is the sum of the actions separately due to the curvatures of its two faces. On the other hand, we know that, in the case of a full liquid mass, the capillary pressure exerted by the liquid on itself emanates from all points of a superficial stratum having as its thickness the radius of activity in question. Now, if the thickness of the film which constitutes a bubble is everywhere superior or equal to double that radius, each of the two faces of the film will have its superficial stratum unimpaired, and the pressure exerted on the enclosed air will have the value indicated by our formula. But if, at all its points, the film has a thickness inferior to or double this radius, the two superficial strata have not their complete thickness, and the number of molecules comprised in each of them being thus lessened, these two strata must necessarily exert actions less strong, and consequently the sum of these, that is to say, the pressure on the interior air, must be smaller than the formula indicates it to be. Hence it follows that if, in the experiments described above, the thickness of the films which formed the bubbles had, through the whole extent of these last, descended below the limit in question, the results would have been too small, but in this case we should have remarked progressive and continued diminutions in the pressures, which, however, never happened, although the color of the bubbles evinced great tenuity. But all physicists admit that the radius of sensible activity of the molecular attraction is excessively minute.

"But what precedes permits of our going further, and deducing from experiment a datum on the value of the radius of sensible activity, at least in the glyceric liquid. * * *

* * * * * * * *

"After the film has acquired a uniform thinness, if the pressure exerted on the air within the bubble underwent a diminution, this would be evinced by the manometer, and it would be seen to progress in a continuous manner in proportion to the ulterior attenuation of the film. In this case the thickness which the film had when the diminution of pressure commenced would be determined by the tinge which the central space presented at that moment, and the half of that thickness would be the value of the radius of sensible activity of the molecular attraction. If, on the contrary, the pressure remains constant until the disappearance of the bubble, we may infer from the tint of the central space the final thickness of the film, and

the half of this thickness will constitute at least a limit, very little below which is to be found the radius in question.

* * * * * * * *

* * * I deposited in the bottom of the dry jar, morsels of caustic potash, and contrived by the application of a little lard around the orifice of the jar and of the aperture through which passed the copper tube, that after the introduction of the bubble, the pasteboard disk should close the opening hermetically. * *

* * * * * * * *

"Now, under these conditions, the diminution of thickness of the film was continuous, the bubble lasted for nearly three days, and when it burst, it had arrived at the transition from the yellow to the white of the first order; it then presented a central space of a pale yellow tint, surrounded by a white ring.

* * * * * * * *

* * * if the pressure varied, it was in an irregular manner, in both directions, and terminating not in a diminution, but an augmentation, at least relative; we may, therefore, admit, I think, that the final thickness of the film was still superior to double the radius of sensible activity of the molecular attraction.

"Let us now see what we may deduce from this last experiment. According to the table given by Newton, the thickness of a film of pure water which reflects the yellow of the first order is, in millionths of an English inch, $5\frac{1}{4}$, or 5·333, and for the white of the same order $3\frac{7}{8}$, or 3·875. We may therefore take the mean, namely 4·064, as the closely approximative value of the thickness corresponding, at least in the case of pure water, to the transition between those colors, and the English inch being equal to 25·4 millimètres, this thickness is equivalent to $\frac{1}{6254}$ of a millimètre. Now we know that, for two different substances, the thickness of the films which reflect the same tint is in the inverse ratio of the indices of refraction of those substances. In order, therefore, to obtain the real thickness of our film of glyceric liquid, it suffices to multiply the denominator of the preceding fraction by the ratio of the index of the glyceric liquid to that of water. I have measured the former approximatively by means of a hollow prism, and have found it equal to 1·377. That of water being 1·336, there results, for the thickness of the glyceric film $\frac{1}{8611}$ of a millimètre. The half of this quantity, or $\frac{1}{17222}$ of a millimètre, constitutes, therefore, the limit furnished by the experiment in question. Hence we arrive at the

very probable conclusion, that in the glyceric liquid the radius of sensible activity of the molecular attraction is less than $\frac{1}{17000}$ of a millimètre.

"I had proposed to continue this investigation with a view to reach, if possible, the black tint, and to elucidate the variations of the manometer; but the cold season has intervened, diminishing the persistence of the bubbles, and I have been forced to postpone attempts to a more favorable period."

In the report of the transactions of the Society of Physics and Natural History, of Geneva, 1862, we find the following: "Prof. Wartmann, Jr., repeated before the Society the recent experiments of M. Plateau on bubbles of soap, of varied forms as well as much persistency, obtained by mixing with soap-suds a small quantity of glycerine, and causing the bubbles to attach themselves to iron wires arranged in different manners. At a subsequent session M. Wartmann exhibited an apparatus of the same kind, still more varied, so as to produce more perfectly than by former processes the phenomena of coloration in extremely thin surfaces of the liquid. The dark part presents not more than $\frac{1}{1919}$ of a millimètre, whence we may conclude, says M. Wartmann, that the radius of the sensible activity of molecular attraction is below $\frac{1}{209000}$ of a millimètre."

CAN WE RAISE OUR OWN SUGAR?*

By H. W. Bartol, Esq.

Can we raise our own sugar? This question having often been asked me, and various articles having appeared in the papers on beet sugar, I have thought that a few words as to how it is made in Europe, and the probable success it would meet with in this country might not be uninteresting to the community at large.

Cane sugar was first discovered in the beet, in the year 1747, by a German chemist, named Margraf; and, in 1796, M. Achard analyzed the beet, and obtained 5 per cent. of white sugar; but it was not until the rule of Napoleon I., who offered a prize of $200,000 for the best method of producing sugar from native productions, that beet-root sugar-making reached any importance.

The discovery thus made and put into practical operation, was

* In all places where I speak of France, the money referred to is gold; but wherever this country is referred to greenbacks are understood to be the standard.

nursed by enormous protective duties, which, at one time, raised the price of sugar as high as $1.25 per pound.

Under this state of affairs, sugar-making became an important branch of industry, and many improvements were made by which the yield of sugar, which at first was only 3 per cent., was augmented to 5; this was the state of affairs at the time of the restoration, when the duties being removed, sugar-making from beets, in France, became a thing of the past.

Beet-root sugar-making remained dormant, as it were, for some time: but a great industry can no more be crushed than a great truth, and Napoleon III. seeing this, commenced a judicious course of protection, which has brought beet-root sugar-making to its present state of perfection.

Science having increased the yield of sugar from 3 to 8 per cent., (and in some factories as high as 9 and 10 per cent.,) and improved machinery diminishing the cost of production, so that to-day beet sugar not only stands on a par with that made from cane, but actually drives it out of the European market, many refiners in England (where all foreign sugar enters on the same terms,) using it entirely, and preferring it to that made from cane, it being much clearer.

Can beet sugar be made in this country? Let us look into the matter, and decide for ourselves.

The best white sugar (equal to Lovering's A,) is sold in France at 11·5 cents per pound, from which we must deduct the Government tax of 4·09 cents, leaving the manufacturer 6·41 cents per pound to pay all his expenses and make his profit from.

If the French manufacturer can sell his sugar at that price, surely we can make it at double the price, and sell it at 16 cents, leaving a clear profit of 3·16 cents per pound; and in a factory working sixty tons of beets in twenty-four hours, (the smallest size factory it is profitable to run,) yielding 7 per cent. of sugar, we would have a profit of $265.44 per day.

In order that you may the better understand the terms I may hereafter use, I will give a short description of how beet sugar is made.

The beets being brought to the factory, are thrown into a large, revolving cylinder, set on an angle, and immersed one-half in running water, in which, by means of cams, they are gradually worked to the upper end and thrown out clean on to a revolving table, around which stand a number of women, who take off the beets

and cut out the bad parts, after which the beets are conveyed to the rasp.

This machine is a drum, encased in a cylinder, in which it revolves; the circumference of the drum is composed of a series of saw plates, set in it radially, with the teeth out, so as to form a number of sharp points; this drum revolves with great rapidity, so that the beets, which are introduced at the top with 30 per cent. of water, are immediately grated, and pass out at the bottom in the form of fine pulp.

And now comes the point of dispute. There are three ways of extracting the juice from the pulp, viz: 1. Hydraulic Presses; 2. Centrifugals; 3. Macerators..

For brevity's sake, I will merely mention the first two, the last being very little used.

In the press process, the pulp is put in bags, and subjected to a great pressure in hydraulic presses, which expresses the juice, and leaves a dry mass.* Where fuel and water are scarce, it is imperative to use this process, as it requires less of both than any other way, and, in fact, is the most generally used.

By the centrifugal process, the pulp is placed in vertical wire cylinders, about 3 feet in diameter, 18 inches high, revolving with a velocity of 1200 revolutions per minute, which, by its centrifugal action, drives the pulp with great force against the wire sides, and they being too finely woven to allow the pulp to pass through, retain it, and let the juice flow off, when it is caught by an exterior cylinder, and conveyed by canals to its proper receptacle.

This process has the advantage of requiring 50 per cent. less manual labor than the presses, but requires a large amount of additional water, (40 per cent.) which is used in washing the juice out of the pulp; while in the centrifugal machine, which water, in its turn, thins the juice, so that it requires more fuel to evaporate it.

There is another process coming into vogue, in Germany, called the "Roberts' Diffusing Process," but, as it is a new thing, and not fully proved, I refrain from speaking of it.

The juice, obtained by any of these processes, is taken to what is called a decarbonizer, which is really nothing but an iron tank, fitted with two sets of pipes, one for steam and the other for carbonic acid gas.

* The dry pulp is sold to feed cattle, at the same price that is paid for the beets.

In this vessel the juice is heated to 180°, and milk of lime added until the liquor is strongly alkaline; it is then neutralized by forcing in carbonic acid gas, which, uniting with the lime, forms a a precipitate, which, in settling, carries down with it many of the impurities held in suspension.

The juice is allowed to settle, when the clear portions, of a light straw color, is drawn off, while the settlings are passed through filter-presses, which extract the clear juice, and leave a dry mass, composed principally of carbonate of lime.

The process, from this out, is the same as that pursued with cane sugar. The juice is boiled down to the required density, (27° Beaumé,) passed over animal black, re-boiled to grain, and put into small tanks where, after remaining a few days to crystalize, it is passed through centrifugal machines, in the ordinary way, and comes out dry sugar.

But why should it cost us twice as much as the French to make sugar, as the necessary articles to make it are certainly not twice as dear in this country, with the one exception of labor; and if it can be shown that labor makes not quite one-third of the expense on a pound of beet sugar, and that we can and have raised beets in this country as cheaply as in France*, it seems to me that all doubt on the subject is removed; to prove this, I refer to the following tables.

The following is the table of the working of a French house, and all the figures used in it are strictly correct. The profit named is very low, in order to make the working expenses as heavy as possible. The 10 per cent. loss in the beets is the bad parts, which are cut out, as before mentioned.

EXPENSES.

60 tons of beets, at $3.75	$225 00
Running expenses	273 90
Gov't tax at 4 09 cents per lb. in 8,400 lbs. sugar	353 86
Total	$852 76

* Mr. Gennert, Superintendent of the Germania Beet Sugar Company, of Chattsworth, Illinois, assures me that his average yield per acre, in fair years, is fifteen tons of beets, containing 12 per cent of saccharine matter, and that the cost of raising them, everything included, is not quite $3.00 per ton. In France, the tax being on the sugar, they average eighteen tons, containing from 10 to 12 per cent.; while in Germany, where the tax is on the beets, they endeavor to get the most sugar in the fewest beets, and, consequently, do not average more than fifteen tons, containing from 12 to 15 per cent. of sugar.

RECEIPTS.

7 per cent. of sugar on 60 tons beets less 10 p. ct. loss, 8,400 lbs. sugar at $.103	$865 20
15 per cent. pulp, = 9 tons at $3.75	32 44
Total	$897 64
Less expenses	852 76
Profit per day	$44 88

In this calculation, I have left out a small amount of syrup, as the probabilities are that it would be impossible to sell it in this country, on account ef its extremely unpleasant taste. In France, it is made into alcohol.

Having now arrived at the cost of manufacture in France, let us see how we would come out in America, where the cost of labor is about twice as great. (In France a man receives sixty cents, gold, per day; in this country, a man for the same work can be hired for about double that amount, in greenbacks.* It must be remembered that these are country wages, and always lower than city prices.)

In the following tables, I have put the beets at $2.00 per ton† higher than Mr. Gennert makes them, as my object is not so much to show the exact amount of profit to be derived in this country as to establish, beyond a doubt, that we *can raise our own sugar.*

Factory working 60 tons of beets per day in America:

EXPENSES IN GREENBACKS.

60 tons of beets, at $5.00	$300 00
Expenses of running double those in France	547 80
Total	$847 80

RECEIPTS.

Sugar, 7 p. ct. on 60 tons, less 10 p. c. =8,400 lbs., at 15 cts	$1,260 00
15 per cent. pulp, $5 per ton	45 00
Total	$1,305 00
Less expenses	847 80
Profits per day	$457 20

* See Report of Commission of Agriculture for June, 1866.

† From all the data I can collect, beets can be raised as cheaply as corn, and the Commissioner of Agriculture, in his report for June, 1866, gives as the average crop of corn, in twenty-two States of the Union, from the year 1862 to 1865, inclusive, as 32 99 bushels per acre,

Which, at $1.00 per bushel, (a very high price,) would be	$32 99
While 15 tons of beets, at $5.00 per ton, would give	75 00
Balance in favor of beet grower	$43 01

As to the richness of our beets in sugar, I refer to the following analysis made by me:

	DATE.	PER CENT. OF SUGAR.
Beets grown at Ellwood, New Jersey	Nov. 3, 1867.	12
" " " "	" 7, 1867.	12
" near Boston, Mass	" 15, 1867.	11·40
" by Germania Sugar Co	Jan. 15, 1868.	12·00

Most of these beets were grown by persons having no knowledge of raising them for sugar making.

Few people are aware of the great extent to which beet-root sugar is made in Europe, I therefore give the following table, extracted from the *Journal des Fabricants des Sucre*, giving the amount of sugar made in Europe during the season of 1867 and 1868. (The sugar-making season commences October 1st, and ends April 1st.)

	Tons.
Associated Germany	165,000
France	220,000
Russia	97,500
Belgium	32,500
Poland and Sweden	15,000
Holland	7,500
Austria	92,500
Total	630,000

If, then, Europe can raise her own sugar, why cannot we, and keep at home, to enrich our own land, the $80,000,000 of gold we annually send to foreign countries for that article?

If necessary, let the Government imitate Napoleon I. by offering large prizes for the first successful establishment, so that, if, in the future, the much-vexed question is asked, "Can We Raise Our Own Sugar?" we may respond with pride, "We can, and do!"

H. W. Bartol.

INVERTED SYPHONS IN THE SEINE.

SINCE the middle of last month, the works belonging to the new drainage scheme have advanced to such a pitch that to complete them it was necessary to get the syphons sunk, which acted as the general collector of the storm and other waters draining from the left bank of the Seine. The syphons themselves, consisting of wrought iron cylindrical tubes, are first rendered completely water-tight and then immersed in the river, where they float half in and half out of the water. To accomplish this operation successfully, a certain amount of current is required, as well at a certain depth of Water, in order to bring them right over that portion of the bed of the river which will constitute their ultimate site. The first tube having been hauled perpendicularly over the proper spot, was uncorked, if we may use the term, and, after gradually filling, descended quietly into its place without the slightest mishap of any kind. The success which attended the sinking of the first syphon was hailed as a good omen for that impending over its neighbor, but unfortunately some time elapsed before the omen was fulfilled. Upon the day fixed for its immersion, the velocity of the river had increased, and the level of its surface had fallen 6½ feet. The cause was the bursting of the dam of Grande-Jatte, at Neuilly, which, together with the increase of the velocity of the current, put an end to the preparations for sinking the second tube. It was accordingly towed to a place of safety, and made fast. As it was impossible to repair the breach made in the dam at Neuilly, and as the navigation was seriously interfered with by the sudden lowering of the water, recourse was had to the weir at the bridge of Jena, which was purposely adapted for such contingencies. By its means a sufficient head of water was obtained, and the operation of placing the second tube alongside its predecessor proceeded with. The cylinders, of wrought iron, with countersunk rivets, were constructed at the establishment of Chaillot, and their total length from wall to wall of the river is about 450 feet. After being sunk, they will be encased in an envelope of bêton, 16 inches thick, to protect them from the heaving of anchors, the action of dredging, or any other cause that might be likely to damage them. There does not seem to be any cogent reason why the cheaper material, cast iron, should not have been employed in lieu of wrought in the construction of these tubes. They have no weight to sustain, no real work to do. We should have regarded an instance of this kind as one peculiarly adapted for the employment of cast iron, but judging from this case and that of the late exposition, that material is manifestly not a favorite with continental engineers.—*Mechanics' Magazine.*

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

A Lecture delivered at the Academy of Music, before the Franklin Institute, on May 23d and June 6th, 1868.

By Prof. Henry Morton, Ph. D.

(Continued from page 351.)

We propose here to place a section which might be considered as a long note, since it contains matter not developed in the lecture, but which may, perhaps, be introduced, with propriety, in this connection, as involving that which is worth putting on record at this time, while the events are fresh in memory, and authorities in the subject personally accessible for correction or emendation. We propose, in fact, to give a brief sketch of Celestial Photography, which may place the succession of the laborers in this field in true order, and afford a guide to the literature of the subject.

The first photograph of a celestial object was taken by Dr. J. W. Draper, of New York, in 1840. This was followed by Prof. G. P. Bond, of Cambridge, U. S., who, in 1850, made a picture of the moon on a daguerreotype-plate, in the great Cambridge refractor of 15-inch aperture.

During the years which succeeded, there appeared in the field of Celestial Photography, Father Secchi, in Rome, Birch and Arnaud, in France, and Phillips, Hartnup, Crookes, De la Rue, Fry, Dancer, Huggins, Williamson and Baxendell. The first detailed description of experiments was published by Professor Phillips, who read a paper on this subject before the British Association in 1853. Professor Phillips made his pictures with a 6¼-inch refractor by Cooke, of 11 feet focus, which produced negatives of the moon 1¼ inches in diameter, in 30 seconds. A paper was next communicated to the Royal Society, by Crookes, in December of 1856; he subsequently obtained dense negatives in 4 seconds. In May, 1857, Mr. Grubb read a paper on this subject before the Dublin Photographic Society.

In 1859, De la Rue read a paper before the British Association. His experiments were commenced in 1852, with a reflecting telescope of his own manufacture.

In 1860, De la Rue and his assistants made several excellent negatives of the solar eclipse; and he was much interested in the establishment of the Photoheliograph at Kew, with which pictures of the sun, showing the "spots" and their changes, have been made almost daily since.

In the *Quarterly Journal of Science*, for April, 1864, appeared a paper by Professor Henry Draper, M. D., in which he describes the progress of his experiments in constructing a reflecting telescope of glass, silvered on the first surface, by Foucault's plan, designed especially for the purposes of Celestial Photography. After grinding and polishing more than one hundred experimental mirrors, the art was at last fully acquired, and the work is now, as Dr. Draper has informed us, a certain and not very lengthy process.

The mirror, which was finally mounted, was of 15½ inches diameter and 12½ feet focal length, and with this 1,500 negatives have been taken, of which very many are of great excellence. We have now before us a print, on paper, from one of these negatives, showing the moon in the third quarter, with a diameter of 30 inches, and we have seen one reaching the enormous size of *five feet.*

In *Silliman's Journal*, for May, 1865, Mr. Louis M. Rutherfurd gives an account of his labors in this field, in the first place with a refractor and a silvered glass reflector of the usual construction, and then with a refractor made by himself, and corrected only for the chemical rays. This instrument was completed in December, 1864, but it was only in March, 1865, that the atmospheric conditions were sufficiently favorable to admit of a trial.

We have before us a set of four pictures, bearing date respectively, March 4th, 6th, 8th and 10th, which are the work of this instrument, and about which (mistrusting our own judgment where a fellow-countryman is concerned,) we will quote the expression given by Mr. Brothers, an eminent English laborer in the same field, who, in a chapter written for Mr. G. F. Chambers' admirable Astronomy, just published, says,*—"To an American (Mr. Rutherfurd,) we are indebted for the best photograph of our satellite yet produced, and, indeed, it is difficult to conceive that anything superior can ever be obtained.

(To be continued.)

* To this chapter by Mr. Brothers, we owe most of the matter given above, endorsed, however, by a direct communication with Mr. Rutherfurd and Dr. Draper.

LECTURES ON VENTILATION.

By Lewis W. Leeds.

Second Course, delivered before the Franklin Institute, during the winter of 1867–68.

(Continued from page 355.)

LECTURE II.

The air we breathe—what is it? Oxygen and nitrogen, the young student will promptly answer. Oxygen and nitrogen certainly; but how many of us really comprehend what these elements are?

We are told that the distance around the earth is *so* many thousand miles, and so many more to the sun, and that the nearest fixed star is a great many millions of miles from us; but it is no matter how many (though the astronomers have stated it all with accuracy), because I believe it is entirely impossible for the mind of man to realize or in any way to imagine, more than an exceedingly small fraction of that distance. His imagination will, I think, in all such attempts, be very much limited by the greatest distance he may at some time have seen with his natural eyes.

And when the microscopist tells us of the immense number of living beings found in a drop of water, and when we must know that these diminutive creatures necessarily possess—for performing their functions of life—much of the exquisite machinery of the larger animals, how entirely incapable are we of extending our imaginations to any satisfactory apprehension of the minuteness of these things.

Thus, in whichever way we may turn our investigations, we can find no limit to the minuteness on the one hand, nor to the greatness of creation on the other.

Were we to attempt the study of a single blade of *grass*, and endeavor to learn the many combinations of the simple materials constantly surrounding us which enter into its composition; or were we to try to comprehend the power of that wonderful substance we call *heat*, its source and the cause of its undiminished supply, we should find it the study of a lifetime.

Some of the experiments we may show you this evening, may be as familiar to you as the reflection of your own face in the glass,

yet they may appear to you in a *new combination*, and possibly suggest to you some new thoughts.

You may have attended a lecture on Chemistry only last night, and have been delighted with the most beautiful and elegant discourse on the all-powerful and wonderful properties of that greatest of substances—Oxygen—and have seen with what ease the lecturer could build up and pull down all things earthly that man beholds. Or you may have followed the *Geologist* in his deep and profound researches into the very bowels of the earth, and listened with rapture as he explained the wonderful formations and transformations which these same substances have been undergoing for untold ages. Or perhaps you may have attempted to follow the *Astronomer* in his daring flights of fancy, as he endeavored to explain to you how that wonderful heat and light of the sun are kept up by the millions upon millions of meteors, which, drawn irresistibly from their paths, dash headlong into that great consuming fire.

But this evening, let us concentrate our thoughts much nearer home; let us endeavor to comprehend more fully the most interesting to us of *all* those mysterious changes; I mean that change which is produced in our own bodies twenty times every moment of our lives, by the air we breathe.

I am led to believe that we have not more fully comprehended this mysterious transformation thus going on within our own bodies, than we have truly measured *mentally* the distances to the fixed stars, or the number of days and hours since the formation of the earth, or can compass by our limited reasoning, those powerful influences which have caused the sun to send forth heat and light without measure, for unnumbered ages, and yet the source itself to be undiminished still.

I say, I am led to the belief of this general want of knowledge upon this subject, by noticing daily, and almost every hour of the day, the most intelligent and best educated men and women amongst us, so entirely ignoring the effect designed by our Creator to be produced by this constant breathing of pure and fresh oxygen and nitrogen, as to shut themselves up in *close rooms*, and breathe and rebreathe the same air, till it becomes excessively foul and poisonous.

There is no standard of taste respecting things seen. Those which we know produce the most comfort, and give us the most happiness, no matter what may be their form, we consider the most beautiful.

We cannot *see* pure air, and therefore we can scarcely call it "beautiful;" but let us hope that we may so cultivate the imagination, that if we can but comprehend, even in a slight degree, the marvellous effect that it produces on all that is human, we may soon learn to *feel* that pure air was not only among the greatest, but that it was *the* greatest of all the Creator's temporal blessings to his creature, man.

At our last lecture, I endeavored to impress upon your minds, the very large amount of air which was breathed by each individual every twenty-four hours, it being about 350 cubic feet, or 125 times our own bulk.

Now if all this great bulk was simply *nothing*, or possessed no qualities except what we could discern with our ordinary vision, it might be of little value. Our eyes are the great medium through which we receive information; but, as I said before, we cannot *see* the atmosphere, and so I fear many of us fail to appreciate the true value of pure air on this account. We are obliged, therefore, to explain its peculiarities in a secondary manner, by producing some *effect* that may be seen.

The air we breathe is composed of seventy-nine parts by bulk of nitrogen, and twenty-one parts of oxygen.

The oxygen is the *busy body;* it is the hard working, active substance that keeps up the fires, cooks the food, burns up the trash, purifies the blood and turns it from a dark purple to a bright crimson color.

[Here followed the ordinary experiments of burning sulphur and phosphorus in oxygen, those elements being consumed with great rapidity and brilliancy.] Thus you see some of the effects that may be produced with pure oxygen, which forms about one-fifth of the air, when it is separated from the nitrogen forming the other four-fifths of the ordinary atmosphere. The nitrogen seems to be a mere dilutant to keep the oxygen under control, and to prevent it from burning or destroying everything by fire or rust.

[A second experiment was here introduced, to show the indispensability of oxygen, and was explained as follows:]

Here we have an ordinary candle, which you see burns just as candles generally do; but now let us place it under this small bell glass receiver, and observe how rapidly the flame diminishes in size.

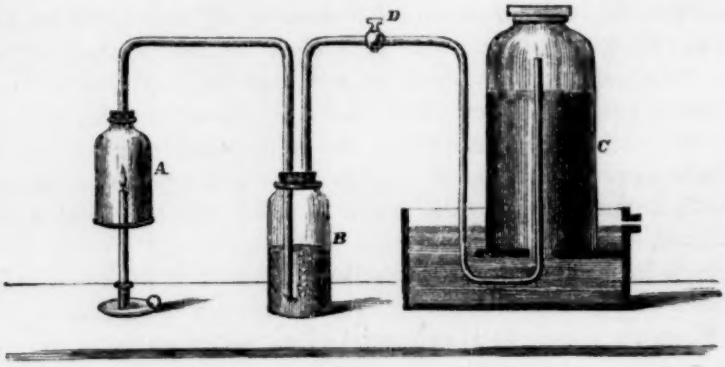
This next glass jar (B), contains lime water, or if it should chance

to be Goulard's Extract of Lead, it would answer the same purpose; and you will please notice that it is at present perfectly transparent.

From the top of this wash-bottle, we have a connecting tube passing down under and extending up again to the top of this glass jar (C), which is now filled with water.

You see the candle is now almost entirely extinguished, but please note carefully as I open the stop-cock (D), how quickly the

Fig. 4.



candle gets brighter; and you see also the air bubbling up through the lime water, and in the glass receiver the water gradually falling as the air is admitted to take its place.

The candle is now burning as brightly as though it were in the open air; it has all the oxygen it wants, but let me turn the stop-cock so as nearly to close the current of air, the velocity of which you can measure by the bubbling up of the air through the lime-water. See now how the flame of the candle has diminished! Did you ever see such a beautiful way of regulating the burning of a candle? We thus have as perfect control over it as we have over the gas-light. You will notice that the entire bottom of the bell glass is open, and don't forget that when you are burning much gas, you must have—as this experiment teaches—an outlet for the escape of foul air from the top of the room. The open fire-place, so useful for ventilation during the day, is not sufficient when the gas is lighted.

Ah! see what a change has taken place in this lime-water; it has become white and milky; Also, the water has fallen out of the glass jar, as you see, and drawn in sufficient air to take its place.

Now we are anxious to know what change has taken place in this air. The milky appearance of the lime-water is the chemist's test of carbonic acid; we will therefore assume that that has been caught there, and, as the air has thus been purified again, looks very pure as seen under the gaslight, at any rate. Let us see now if the candle will not burn just as brightly as in the open air.

Why, what can be the matter? It goes out as suddenly as if it was dropped into the water. And for what reason? It is simply because the oxygen is all burned out, and there is nothing but the nitrogen left, and that is entirely incapable of supporting combustion. By looking at this candle, you see we have scarcely burned the little cone at the top, and yet even that has produced sufficient carbonic acid to discolor this bottle of lime-water, and burn all the oxygen out of the large jar of air so thoroughly that it will no longer support the flame of a candle. An ordinary gas-burner consumes as much oxygen and forms as much carbonic acid as five persons.

Now let us see how nearly the burning of the fires in our bodies corresponds with that of the candle we have just examined.

First, let us take another bottle of lime water similar to the one we have just used, and by *inhaling* through the short tube, I draw the atmospheric air through the lime-water; but, as you perceive, although I have inhaled many times, there is no discoloration of the contents. I fear, however, if I was to continue much longer drawing the air from this (as I must confess), unventilated room, with all these gas-lights, as well as human fires burning, I would soon produce an evident discoloration; but I will reverse the preceding operation, and force my breath through the long tube, and you now see how quickly that causes the same milky appearance which resulted from the air coming from the candle.

Let us proceed now to detach this tube, and by drawing the air out of the glass receiver (C), you know that the water will flow in to take its place, even if it was thirty-two feet high. We will notice particularly the number of exhalations required to fill the jar with my breath, and likewise the number of seconds. There, you see it has taken nine exhalations, and required thirty-two seconds, and that jar, as I know, holds six quarts, or one and one-half gallons which would be represented by 346 cubic inches. This is quite excessive, as it would be at the rate of over 600 cubic inches per minute, and you remember we found only 400 the other evening as the average; it may not, therefore, show the ordinary signs of pol-

lution, but let us try it with a candle. Ah! you see it is too foul altogether to support the combustion of a candle.

I wish I could tell you something about the composition of that jar of air; but really, I know nothing about it as I ought to know, nor does any one else. The doctors tell us that one-half of all who die are killed by foul air; but I believe there is not a medical college in the whole country that pretends to teach the careful analysis of this said air. They will spend month after month discussing, and write volume upon volume to prove, that the ten-thousandth part of one drop of medicine will have more effect than a whole spoonful; but as to teaching the analysis of anything quite so common as the air we breathe, that would be too commonplace entirely; so I must simply repeat, we know nothing about it. But there have been a few Germans and Swiss, and some others, who have been able to tear themselves away from the fascinating study of medicine, long enough to make a few preliminary experiments on the more urgently important subject of the air we breathe, and to endeavor to ascertain the effect of the air upon the body, and, reciprocally, of the body upon the air.

(To be continued.)

Franklin Institute.

Proceedings of the Stated Monthly Meeting, October 21st, 1868.

THE meeting was called to order with the Vice-President, Mr. Coleman Sellers, in the Chair.

The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations to the Library received at their stated meeting held October, 14th inst., from the Society of Arts, London, and the Association for the Prevention of Steam Boiler Explosions, Manchester, England; l'Academie des Sciences, l'Ecole des Mines, Paris; la Société Industrielle, Mulhouse, France; der K. K. Geologischen, Reichsanstalt, Vienna, Austria; and Samuel Hart, Esq., Philadelphia.

The various Standing Committees reported their minutes.

The report of the Resident Secretary, on Novelties in Science and Arts was read, after which the meeting, on motion, adjourned.

HENRY MORTON, *Secretary.*

Bibliographical Notices.

Sloans' Architectural Review and Builders' Journal. Claxton, Remsen & Haffelfinger, Publishers, Philadelphia.

We have received the first three monthly numbers of this Journal, which we have read with interest, and can conscientiously commend in the highest degree.

The illustrations in this Journal are one of its most valuable features, and consist of ground plans, elevations, perspective views, and details of construction, all admirably executed and of great use to those for whose benefit this publication is intended. The entire work is eminently creditable to its publishers, and we wish them all success.

A Treatise on Algebra. By Elias Loomis, L.L.D., Professor of Natural Philosophy and Astronomy in Yale College. Revised edition. Harper Bros., New York. For sale by J. B. Lippincott & Co., Philadelphia.

With reference to such a work as this, by an author so widely and favorably known, it would be superfluous for us to occupy space by an extended description or criticism. We believe that we cannot do better in the interests of our readers, than quote a portion of the author's preface, in which he describes the distinctive points of the present edition of this well-known work.

"The stereotype plates of my Treatise on Algebra having become so much worn in the printing of more than 60,000 copies that it had become necessary to cast them aside, I decided to improve the opportunity to make a thorough revision of the work. I therefore solicited criticisms from several college professors who had had much experience in the use of this book, and, in reply, have received numerous suggestions. The book has been almost entirely re-written, nearly every page of it having been given to the printer in manuscript. The general plan of the original work has not been materially altered, but the changes of arrangement and of execution are numerous. In the former editions, in place of abstruse demonstrations, I sometimes employed numerical illustrations, or deducting from particular examples. In the present edition, such methods have been discarded, and I have aimed to demonstrate, with conciseness and elegance, every principle which is propounded."

A COMPARISON *of some of the Meteorological Phenomena of* OCTOBER, 1868, *with those of* OCTOBER, 1867, *and of the same month for* EIGHTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	October, 1868.	October, 1867.	October, for 18 years.
Thermometer—Highest—degree	70·00°	80·00°	90·00°
" date	11th.	19th.	4th, '58.
Warmest day—mean	63·17	70·00	78·30
" " date	11th.	19th.	6th, '61.
Lowest—degree	34·00	38·00	28·00
" date	18th & 24th.	24th & 25th.	25th, '56.
Coldest day—mean	40·33	45·33	35·80
" " date	23d.	24th.	27th, '59.
Mean daily oscillation	12·06	15·39	15·01
" " range	5·12	5·28	5·40
Means at 7 ¼ A. M.	48·92	50·71	51·18
" 2 P. M.	58·24	63·55	62·60
" 9 P. M.	52·92	55·79	55·21
" for the month	53·36	56·68	56·33
Barometer—Highest—inches	30·552	30·465	30·552
" date	30th.	24th.	30th, '68.
Greatest mean daily pressure	30·541	30·456	30·541
" " " date	30th.	24th.	30th, '68.
Lowest—inches	29·685	29·556	29·012
" date	5th.	10th.	26th, '57.
Least mean daily pressure	29·740	29·594	29·059
" " " date	5th.	11th.	26th, '57.
Mean daily range	0·190	0·175	0·148
Means at 7 A. M	30·144	30·053	29·929
" 2 P. M	30·105	30·005	29·886
" 9 P. M	30·122	30·020	29·913
" for the month	30·124	30·026	29·909
Force of Vapor—Greatest—inches	0·534	0·604	0·731
" date	3d.	5th.	7th, '61.
Least—inches	·094	·142	·065
" date	17th.	24th.	21st, '59.
Means at 7 A. M	·281	·315	·313
" 2 P. M	·304	·339	·337
" 9 P. M	·303	·350	·325
" for the month	·296	·335	·325
Relative Humidity—Greatest—per cent	91·0	100·0	100·0
" date	5th.	29th.	29th, '67.
Least—per cent	28·0	33·0	23·0
" date	17th.	7th & 8th.	21st, '59.
Means at 7 A. M	76·8	81·3	77·9
" 2 P. M	59·6	55·3	56·1
" 9 P. M	72·3	75·9	73·1
" for the month	69·6	70·8	69·0
Clouds—Number of clear days*	5·	12·	9·7
" cloudy days	26·	19·	21·3
Means of sky covered at 7 A. M	65·2	54·8 per ct	57·0 per ct
" " " 2 P. M	62·9	44·2	55·5
" " " 9 P. M	54·5	40·3	40·8
" " for the month	60·9	46·4	51·1
Rain—Amount—inches	2·240	4·020	2·951
No. of days on which rain fell	11·	7·	8·8
Prevailing Winds—Times in 1000	N22°18′W.241	N76°37′W.257	N76°38′W.233

* Sky one-third or less covered at the hours of observation.